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The Impact of Social Context on Conservation Auctions:
Social Capital, Leadership and Crowding Out

by

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Abstract

Conservation auctions are a policy tool that can be used to cost-effectively achieve environmental goals, by providing incentives for landowners to adopt environmentally friendly beneficial management practices (BMPs) on their land. Using a competitive bidding process, the party interested in encouraging BMP adoption selects and enters into contract with landowners, who receive monetary compensation in return for their adoption of the new practices.

Previous research on conservation auctions has focused mainly on various design choices which can impact auction performance. This study takes a different approach, examining the influence of the social context in which auctions take place.

Real auctions may be implemented in communities with varying levels of social capital and leadership. Since these factors have been shown to influence individual behaviour in a variety of settings, we hypothesize that they may also influence bidder behaviour within a conservation auction and, in turn, the cost-effectiveness and environmental outcomes of the auction. Using simulated auctions in an experimental setting, we sort participants into experimental treatments based on social capital and leadership characteristics. We find that both social capital and leadership do indeed have multi-dimensional, context-specific effects on bidder behaviour and auction outcomes.

In addition, real auctions may take place in communities where some landowners have already adopted BMPs, driven by pro-social or pro-

environmental “internal” motivations. Previous research has found that such motivations may be crowded out by the introduction of “external” motivations such as fines or regulation. We show, using an experimental approach, that conservation auctions also appear to cause crowding out of voluntary pro-environmental behaviour.

This research contributes to the academic literature by linking theories of social capital and leadership to the literature on conservation auctions, and extending the literature on crowding out to this specific policy mechanism. In addition, it provides an innovative way of investigating the influence of social factors within an experimental setting. There are also important policy implications, as our findings draw attention to the importance of considering social context when designing and implementing conservation auctions.

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Chapter 1: Introduction

Conservation auctions are a promising new tool for environmental management that have drawn attention from both policy-makers and academic researchers in the past several years (Latacz-Lohmann and Schilizzi 2005). They represent a way to address environmental issues that are affected by the actions of many individual landowners and thus are often difficult to manage, such as non-point source pollution and habitat protection.

Conservation auctions are based on the fact that landowners generate negative externalities[1] if they reduce environmental quality through their production practices. They can reduce or eliminate the externalities, improving environmental quality, by adopting "beneficial management practices" (BMPs). BMPs include actions such as restoring wetlands or habitat, or changing farming practices to reduce agricultural runoff into waterways. However, the adoption of BMPs is often costly to landowners, involving both opportunity costs (lost production and time) and direct costs (materials). Meanwhile, landowners receive at most a portion of the environmental benefits. Therefore, without intervention, landowners may fail to adopt socially desirable BMPs.

One means to address this problem, which gained attention in the 1980s, is agri-environmental contracting, also known as payments for environmental (or ecosystem) services, or PES. In these programs, the government, or another party, such as a forestry company or development agency, offers a fixed payment to landowners in return for the adoption of specific BMPs. These are usually cost-share programs that do not cover the full costs of BMP adoption.

However, contracts for BMPs are subject to a severe difficulty: the purchasing party does not usually know the adoption costs of individual landowners. Furthermore, these costs are generally heterogeneous among landowners since they depend on land characteristics as well as existing management practices. Therefore, payments are set at some uniform estimate of

[1] Negative externalities are negative effects on parties who are not directly involved in a transaction, such as people living downstream from a farmer who pollutes a waterway.

costs, instead of being tailored to the actual adoption costs of landowners. This gives rise to a problem of adverse selection: landowners who need to make fewer changes to their management practices are more likely to participate, since their adoption costs are lower; however, the environmental benefits resulting from their participation are smaller (Latacz-Lohmann and Schilizzi 2005). Thus, these programs are unlikely to be cost-effective.

Conservation auctions have the potential to overcome this problem of asymmetric information, by revealing information about landowner costs. In this type of auction, the government (or other purchasing party) asks landowners to submit bids for a limited number of conservation contracts, which involve either specific BMPs or specific environmental outcomes. The best bids, based on environmental and cost-effectiveness criteria, are selected, and these landowners enter into contract with the government. Latacz-Lohmann and Van der Hamsvoort (1997, 1998) show that since optimal bids are a function of the net costs of adoption, the auction mechanism is able to reveal information about these costs.

However, a number of factors affect the ability of the government to use conservation auctions to cost-effectively achieve environmental goals. One is the design of the auction itself. In the discriminatory price auction analyzed by Latacz-Lohmann and Van der Hamsvoort (1997, 1998), the cost revelation mechanism is imperfect. Because the government does not know their actual costs, bidders have an opportunity to behave strategically and capture information rent[2] by over-bidding relative to costs. Bidders' incentives and abilities to take advantage of this opportunity are affected by a number of design choices including the use of a reserve price, the information offered to landowners, and the criteria used for choosing bids. While a uniform pricing mechanism (where all bidders are all paid the same amount) gives bidders an incentive to bid their true costs, it finally requires overpaying all successful bidders and thus may or may not be an improvement over the discriminatory price auction. Therefore, "success of conservation auctions depends on having a thorough understanding of bidding

[2] Information rent, in this case, is income that the landowner receives over and above the actual costs of adoption, which is possible because the landowner has information about those costs that is unknown to the government.

behaviour and paying close attention to design details" (Latacz-Lohmann and Schilizzi 2005: 2).

A second factor which is likely to influence the success of a conservation auction is the social context in which the auction takes place. Social factors such as norms, reciprocity, leadership, altruism, and fairness have been shown empirically to impact people's behaviour in many different aspects of life, including behaviour related to the environment (e.g. Bouma et al. 2008, Krishna 2001, Nyanena 2006). A number of social factors have been shown to influence BMP adoption decisions (Pannell et al. 2006). Therefore, these factors may also impact landowners' participation in conservation auctions, and thereby influence the cost-effectiveness and environmental outcomes of these auctions.

In fact, it is worth noting that social factors may also influence the more fundamental decision of whether or not to use a conservation auction to address a particular environmental issue. The appropriate policy mechanism for a given situation is dependent on the public and private net benefits of the actions to be undertaken (Pannell 2008). If social factors affect these benefits and costs, they may also affect the choice of policy mechanism. While this study does not address this issue directly, it does call attention to the importance of considering these factors in research on such mechanisms.

The issue of social factors has seen little attention in the literature on conservation auctions, thus far. In part, this may be due to the fact that because of the expense involved with running real conservation auctions, research and test-bedding of these auctions is often carried out through simulated auctions in the laboratory. In this context, concerns for participant anonymity and efforts to control for extraneous factors provide little opportunity to observe the impacts of social factors. However, real conservation auctions take place in real social contexts. Landowners are likely to know each other, observe each other's actions, and interact with each other before, during, and after the program. Therefore, it is important to identify the social factors that may affect behaviour within, and outcomes of, conservation auction programs.

This study examines three elements of the social context that may affect the ability of policy-makers to achieve environmental goals using conservation auctions: social capital, leadership, and the issue of "crowding out." Social capital is a multi-faceted concept that involves connectedness and networks between individuals and groups, trust, reciprocity, and social norms. It is likely to be heterogeneous across different contexts where conservation auctions may be implemented, and therefore may be an important factor influencing different behaviours and success rates from one auction to another. Leadership is another factor that is likely to be heterogeneous across different social contexts, since both the type and strength of leadership may differ across communities. Since leaders influence the behaviour of others through a variety of different mechanisms, leadership may also have an impact on auction success. The issue of crowding out suggests that the introduction of an external financial motivation to adopt BMPs may diminish other motivations for adoption, such as social norms or altruism. This implies that conservation auctions may have a negative impact on voluntary BMP adoption, especially if the program only provides temporary payments.

As in many other studies of conservation auctions, the impact of these social factors is studied using controlled lab experiments. However, unlike existing studies in the literature, these experiments are designed to include mechanisms through which the social factors being investigated may play a role. The experimental treatments simulate the effect of different social contexts by varying levels of social capital and leadership, and allowing socially-motivated behaviour to influence individual decisions. Thus, this research improves our understanding of the social factors affecting behaviour in conservation auctions, which offers guidance to improve the design of such auctions.

We find that both social capital and leadership have multi-dimensional, context-specific effects on bidder behaviour and auction outcomes. Group-level social capital appears to lower bids and positively influence outcomes, while the individual-level social capital scores of people who do not know each other has no discernable impact. However, even in existing groups, it appears that social norms have a strong impact on behaviour only when BMP adoption directly affects

participant outcomes, through group bonuses for achieving an environmental target.

Leadership, too, has complex impacts; overall, leaders tend to raise bids and have a negative effect on auction outcomes, but the channels through which this occurs vary depending on the existence of social networks among participants. In particular, when social connections already exist, collusion ("gaming" the auction) appears as a major concern. The specific mechanisms through which BMP adoption affects participants also appears to have an impact, as the provision of bonus group payments may partially offset the incentive to collude.

In the experiments dealing with crowding out, we find that the introduction and subsequent removal of a conservation auction significantly reduces voluntary provision of environmental quality, via monetary donations to an environmental charity, compared to a control group that does not experience an auction. This suggests that crowding out may indeed be a concern for conservation auctions, and implies that auction designers need to pay attention to social context and motivations to minimize harmful effects on voluntary BMP adoption.

Besides the policy implications, the experiments in this study also play a role in linking the literature on social factors to the literature on market-based instruments. While a number of studies have linked social capital and leadership to conservation decisions, few have looked at market-based instruments and none (to the best of our knowledge) have examined this link for the particular case of conservation auctions. Our approach to studying these issues is also unique, as we use existing characteristics of participants to create the experimental treatments, rather than artificially creating social capital or leadership opportunities through the structure of the experiment.

The crowding out experiment also provides an important contribution to the literature. While this issue has been previously studied (Reeson and Tisdell 2010), our experiment provides a closer parallel to real conservation auctions by using real BMP adoption costs, framing the auction in terms of conservation, and,

most importantly, providing incentives that reflect the fact that most benefits of BMP adoption go not to the landowners themselves, but to wider society.

The next chapter of this thesis provides an overview of conservation auctions by reviewing the existing theoretical and empirical literature. The following three chapters introduce the issues of social capital, leadership, and crowding out, describe the experiments used to examine their potential influences on conservation auctions, and report the experimental results. The resulting conclusions are summarized in the final chapter.

Chapter 2: Conservation Auctions

Conservation auctions have been used in the field for several years, although they are still one of the newer tools for environmental protection. An early example of a conservation auction is the Conservation Reserve Program in the United States, which began in 1986. Another well known auction program is BushTender in Australia, which ran from 2001-2003. A number of other auctions have been employed in countries including Germany and Scotland, as well as several more in Australia. In Canada, the non-profit organization Ducks Unlimited has implemented auctions aimed at conservation easements in the prairie provinces (Brown et al. 2010) and at restoring wetlands on agricultural land in Saskatchewan (Hill et al. 2011). Promising results from several of these programs have generated an increasing interest, among both policy-makers and academics, in researching conservation auction theory, design and outcomes.

Theoretical Literature and Model

There is a well-developed economic literature on auction theory. Unfortunately, most of this literature does not apply to conservation auctions since they have a number of unique characteristics. In standard theory, the Revenue Equivalence Theorem (Myerson 1981, Riley and Samuelson 1981) states that any auction design that satisfies certain basic assumptions will result in the same expected revenue for the seller. However, Latacz-Lohmann and Van der Hamsvoort (1997) argue that conservation auctions violate these assumptions in several ways, making the Revenue Equivalence Theorem, and its implications for optimal auction design, inapplicable. First, the assumption of risk neutrality is violated if landowners are risk-averse. Second, bidding is not symmetric; land characteristics and opportunity costs differ across landowners, meaning that improvements in environmental quality may differ even when bid amounts are identical. Third, payments may depend not only on the bids themselves, but also on other factors such as making payments conditional on environmental

outcomes. Fourth, the cost of bid construction may be nonzero. In addition, conservation auctions involve multiple contracts instead of a single contract. Because of these complexities, existing auction theory cannot determine the optimal design for a conservation auction.

Thus, the major theory paper relevant to this study is that of Latacz-Lohmann and Van der Hamsvoort (1997), who present a model of optimal bidding behaviour developed specifically for conservation auctions. The model is based on the idea that farmers have some expectation of a maximum acceptable bid level. To determine the optimal bid, farmers balance the probability that their bid will be accepted (which is decreasing in the bid level) with the net payoff resulting from the bid (which is increasing in the bid level). To provide a framework for investigating the influence of social factors on bidder behaviour, the model is presented here in detail.

The model assumes that BMP adoption will change the profits from farming. Profits from conventional farming are π_0 and profits from conservation farming (with BMPs) are π_1. The assumption is that $\pi_0 > \pi_1$; otherwise profit-maximizing farmers would have adopted BMPs on their own. Profits are an argument in a monotonically increasing, well-behaved utility function U. In the auction, a farmer submits a positive bid amount b. The farmer also has expectations about the maximum bid level that will be accepted, β. He will submit a bid b if:

$$U(\pi_1 + b)Pr(b \leq \beta) + U(\pi_0)[1-Pr(b \leq \beta)] > U(\pi_0) \quad (1)$$

If the farmer's expectations about β are characterized by a probability density function, f(b) and a cumulative density function F(b), then $Pr(b \leq \beta) = 1 - F(b)$. For a risk-averse decision-maker, since conservation practices and auction participation may affect the variability of farming profits, Latacz-Lohmann and Van der Hamsvoort replace the utility function with a certainty equivalent, expected income minus a risk premium RP. This gives the following condition for submitting a bid b:

$$[\pi_1 + b - RP_1(b)][1 - F(b)] + (\pi_0 - RP_0)F(b) > \pi_0 - RP_0 \quad (2)$$

This can be rearranged to:

$$\{[\pi_1 + b - RP_1(b)] - (\pi_0 - RP_0)\}[1 - F(b)] > 0 \quad (3)$$

When the left-hand side of this equation is maximized with respect to b, keeping in mind that it makes no sense for the farmer to submit a bid below the minimum expected bid cap β_{min} or a bid that does not cover the opportunity costs of changing to conservation farming, the optimal bid level is found to be:

$$b^* = \max \{\pi_0 - \pi_1 - [RP_0 - RP_1(b)] + [1 - \partial RP_1(b)/\partial b][(1 - F(b))/f(b)], \beta_{min}) \quad (4)$$

$$\text{s.t. } \pi_1 + b^* - RP_1(b^*) > \pi_0 - RP_0$$

This shows that the optimal bid is increasing in the net opportunity costs of participation, and in the expected bid cap. The model also predicts that risk-averse farmers will offer lower bids than risk-neutral farmers to increase the probability of their bid being accepted, as long as adopting the conservation practices does not significantly increase the variability of profits. Thus, bids will be affected by factors that change the net opportunity costs of participation, expectations about the maximum acceptable bid, and factors that affect the risk attitudes of landowners.

A second theoretical paper outlines a different type of conservation auction, which combines a team contract with an auction (Taylor et al. 2004). In this auction, which is intended for non-point source pollution where individual actions cannot be monitored, landowners submit individual bids for pollution abatement. These bids are used to select participants into the program. Selected landowners then decide how much abatement to actually provide, and payment is contingent on group performance. If the group as a whole achieves the specified environmental target, each participant is paid their individual bid amount; if the target is not met, no one is paid. Taylor, Randall and Sohngen (2003) show theoretically that in this type of auction, the optimal bid level is a function of abatement costs, but again may be an imperfect cost revelation mechanism.

Empirical Literature

The theoretical models described above leave open many questions about the impacts of different auction design features on bidder behaviour and auction performance. However, several design elements have been tested empirically using simulations, experiments, and pilot auctions. They include pricing mechanisms, reserve prices, target constraints vs. budget constraints, information provided to bidders, number of auction rounds, and group vs. individual payments.

Different pricing mechanisms provide different incentives to bidders and thus affect auction outcomes. The two main options are discriminatory pricing, where each successful bidder receives their bid price, and uniform pricing, where each successful bidder is paid the same amount (often the lowest rejected bid). Discriminatory pricing gives participants an incentive to overbid, since selected participants receive their bid amount, while uniform pricing provides an incentive for truthful bids since lower bids are more likely to be accepted. Hailu and Thoyer (2006) confirm this prediction using an agent-based computer simulation. Cason and Gangadharan (2005) also confirm this prediction for individual behaviour; however, in their experimental study, discriminatory pricing resulted in better auction outcomes because more projects could be purchased under the budget cap.

Auction design can also include a reserve price, which is the maximum price per unit that the government is willing to pay. Reserve prices may be announced to bidders, or unannounced. Little research has been published on the effects of this design feature for conservation auctions, although Latacz-Lohmann and van der Hamsvoort's (1997) theoretical analysis indicates that it may be important, if it changes bidder expectations about the maximum accepted bid. Reichelderfer and Boggess (1988), in their analysis of the Conservation Reserve Program in the United States, argue that using alternative bid selection rules instead of the existing strategy of selecting bids on the basis of a reserve price could have improved the cost-effectiveness of the program. However, a reserve price could also be combined with alternative bid selection rules to increase

competition among bidders and to prevent the government from having to pay an amount above society's value of the environmental good being provided.

A third design issue is the choice between a fixed budget and fixed target. In a budget-constrained auction, bids are accepted until the budget for the auction is exhausted. In a target-constrained auction, bids are accepted until a pre-specified environmental target has been met. Schillizzi and Lactacz-Lohman (2007) find that when auctions are repeated, budget-constrained auctions perform better, in terms of cost-effectiveness and minimizing information rents, than target-constrained auctions. More research remains to be done on this issue.

A fourth factor is the information provided to bidders. Glebe (2007) shows theoretically that concealing information about the environmental benefits of adopting new practices from landowners should reduce auction costs, but also reduces the net environmental benefit of the program. Revealing the environmental benefits, but concealing the bid scoring rule, provides the best outcome. Cason, Gangadharan and Duke (2003) confirm empirically that when landowners are informed about the environmental benefits of their actions, they earn more information rent; in this case, abatement is also lower because high quality sellers use this information to place higher bids, some of which are not accepted. Vukina et al (2008) provide an interesting twist on this issue, using data from a real auction to show that when farmers are given information about specific components of the environmental benefits, certain benefits can actually cause them to reduce their bids, since they apparently value these benefits themselves.

Sixth, auctions may be repeated or single-shot. Repeated bidding rounds may be used to allow bidders to become more familiar with the bidding process and the expected benefits and costs of participating; auctions may also be periodically repeated when contracts expire. Hailu and Schilizzi (2005) use an agent-based computer simulation to show that under repetition, auctions may be less efficient and result in less bidder participation than fixed price programs, since bidders are able to extract information rents by using information gained in previous rounds to increase their mark-ups. Similarly, Schilizzi and Latacz-

Lohmann (2007) show that although auctions perform better than fixed-rate payments in a single-shot program, auctions lose their superiority when rounds are repeated. However, Rolfe, Windle and McCosker (2009), using an experiment with landowners as well as data from a real conservation auction, find that holding repeated bidding rounds can increase the efficiency of auctions, suggesting that repetition allows landowners to gain information in a situation where they may be uncertain about costs and returns, and increases competition.

Finally, auctions may be directed towards individuals, or may involve some element of group cooperation. In one type of group auction (Taylor et al. 2003, Taylor et al. 2004), individual bids are used to select participants into the program, while payment is contingent on group performance. If the group as a whole achieves the specified environmental target, each participant is paid their individual bid amount, while if the target is not met, no one is paid. This mechanism can be used in cases where individual compliance is difficult to measure. Taylor et al (2004) show experimentally that this type of auction can be an efficient way to address non-point source pollution. Another variation of cooperative auction is described by Windle et al (2009). In this auction, landowners submit individual bids for taking action on specific land parcels and are paid individually, but the likelihood of a bid being accepted depends on its location relative to other offered land parcels. Over successive bidding rounds, landowners are able to adjust their bids to coordinate with the bids of other landowners.

A further set of studies on conservation auctions deals with cost efficiency, the primary justification of introducing conservation auctions instead of fixed price programs. Glebe (2008) shows theoretically that a bidding mechanism where farmers choose prices and input levels can be more cost-efficient than a self-selection contract mechanism with different levels of fixed payments. Using data from BushTender, a conservation auction carried out in Australia, Stoneham et al (2003) show that the auction mechanism results in significant cost savings relative to a hypothetical fixed-price program. Connor, Ward and Bryan (2008) find the same results based on another Australian auction

program, Catchment Care, while Windle and Rolfe (2008) compare two real programs, an auction mechanism and a fixed price program, and conclude that the auction mechanism is more cost-effective. However, Schilizzi and Latacz-Lohmann (2007) cast some doubt on these conclusions, showing experimentally that although single-shot auctions are more cost-effective than a fixed price program, the fixed price program actually outperforms the auctions under repetition.

Thus, several different design features of conservation auctions have been studied in the literature, both theoretically, experimentally, and using real case studies and pilot auctions. However, to the best of our knowledge, no existing studies address issues related to the social context within which landowners act. Although some auction designs (Taylor et al. 2004, Windle et al. 2009) do allow landowners to interact to some extent, the potential effects of social factors and the connections between landowners on their behaviour in auctions have not been directly studied. However, other research (reviewed in the following chapters) has shown that these factors affect behaviour in many other aspects of life, including conservation behaviour. This suggests that there is a need to study how these factors might influence landowner behaviour within, and the resulting outcomes of, conservation auctions.

Chapter 3: The Influence of Social Capital on Conservation Auctions

Social capital is a multi-faceted concept related to the social networks and connections between people (Pretty and Ward 2001). While social capital has seldom if at all been considered in the conservation auction literature, a variety of studies demonstrate its impacts on behaviour in other aspects of life, and thus suggest that social capital may be important for conservation auctions as well. The experiments described in this chapter investigate this hypothesis.

Economic Definitions and Theories of Social Capital

As yet, there is little agreement among economists on a precise definition of social capital (Hayami 2009). Pretty and Ward (2001) identify four main aspects that summarize common themes in the literature: relations of trust; reciprocity and exchanges; common rules, norms and sanctions; and connectedness, networks, and groups. However, different economists place different emphases on these various themes, and also differ on the key question of whether these elements of social interaction are embedded within communities, or within individuals.

Some economists, following the sociological interpretation of, for example, Putnam (2000), see social capital as interpersonal networks that are embedded in specific communities, meaning that "the same individuals will exhibit different levels and types of social capital depending on the social interactions in which they are engaged" (Bowles and Gintis 2002: F420-21; see also, Wilson 2000, Durlauf and Fafchamps 2005, Dasgupta 2005, Hayami 2009).

Others, however, see social capital as an individual asset, comprising "a person's social characteristics – including social skills, charisma, and the size of his Rolodex – which enables him to reap market and non-market returns from his interactions with others" (Glaeser et al. 2002: F438). In the individual interpretation, these characteristics are transferrable between the different contexts

in which an individual may find himself. This individual concept of social capital was introduced by Loury (1977) and has given rise to a number of economic studies that measure social capital using individual answers to survey questions, finding significant correlations with other individual-level characteristics as well as behaviour (Alesina and La Ferrara 2002, Glaeser et al. 2002, Anderson et al. 2004, Karlan 2005, Kaasa and Parts 2008).

Given this lack of consensus, this study takes a hybrid approach to conceptualizing and operationalizing social capital. For the sake of theoretical analysis, it adopts the individual interpretation of social capital, which makes possible the use of typical economic models of individual behaviour to analyze bidder behaviour in conservation auctions. These theories are outlined below. However, the experimental design allows for both the individual and collective approaches to play a role in creating experimental treatments related to social capital.

Because of the multi-dimensional nature of social capital, economists have tended to develop theories focusing on individual components of social capital instead of an over-arching theory of social capital as a whole. Some of these theories are useful for analyzing the potential impacts of social capital on conservation auctions.

One element of social capital that has attracted attention in the economic literature is social norms. Social norms are unwritten, generally agreed upon rules regarding what behaviour is socially acceptable and what behaviour is not. They are generally modeled by economists as some function of average behaviour or the frequency of a behaviour in a population.

Most theories of social norms (Akerlof 1980, Hollander 1990, Lindbeck et al. 1999, Nyborg and Rege 2003b, Rege 2004, Levitt and List 2007, Nostbakken 2009) are based on the idea that a person who obeys social norms earns social approval from others, and that disobeying social norms results in social disapproval. Social approval and disapproval are assumed to be components in individual utility. A person who acts in a way that obeys the norm thus receives additional utility from her actions, making her more likely to choose

those actions. In these models, the impact of social norms generally depends on the strength of the social norm, the level of compliance with the norm, and a weighting factor that indicates how much the individual cares about following the norm.

Other theories (Brekke et al. 2003, Nyborg et al. 2006) follow a similar argument, but assume that the utility gain from following the norm comes from one's own self-image or perception of oneself as a moral person, rather than the approval or disapproval of others. A related theory is Akerlof and Kranton's (2000) identity model, where social norms are not universal; instead, individuals gain utility from complying with the norms that apply to their own social category. Nevertheless, in both these cases, the source of the norm is the social context.

A second set of theories related to social capital are the theories of reciprocity. These are based on the idea that individuals gain utility from treating others as they have been treated. For example, utility may include a reciprocity term, which multiplies the kindness (positive value) or unkindness (negative value) an individual has been shown by the kindness or unkindness he shows to others (Rabin 1993, Dufwenberg and Kirchsteiger 2004, Falk and Fischbacher 2006). Thus, if an individual feels that he has been treated kindly by another person, his utility is increased by treating the other person kindly; if he has been treated unkindly, his utility is increased by responding unkindly. Reciprocity can also be modeled by assuming that an individual's utility includes the utility of others (altruism), but that the utility of others is weighted, positively or negatively, according to the kindness/unkindness of their actions (Schmid and Robison 1995, Levine 1998, Segal and Sobel 2007).

Because of the multi-faceted nature of social capital, no one theory fully captures all its components. However, the two sets of theories above, those focused on social norms and those focused on reciprocity, provide a starting framework for empirical investigation of the impacts of social capital on individual behaviour, in the context of a conservation auction. The following

section reviews a sampling of the many empirical studies of social capital, with particular attention to its effects on behaviour related to conservation.

Empirical Evidence on Social Capital

Empirical studies on social capital include laboratory studies that create artificial settings to isolate specific components of social capital, and empirical work on real-life situations. Fehr and Falk (2002) provide an overview of some of the experimental work. Studies that use lab games and experiments to show that social norms have an impact on behaviour include Gachter and Fehr (1999), Falk and Fischbacher (2002), Falk et al. (2003), Rege and Telle (2004), Bernhard, et al. (2006), and Lusk and Norwood (2009). Studies showing the impact of reciprocity include Falk et al. (2008) and Charness and Rabin (2002). In addition, Ostrom (2000) interprets commonly observed patterns in public goods games in terms of reciprocity as well as social norms. Lab games have also been used to study other components of social capital, such as trust (Karlan 2005).

It is not immediately clear how these effects, shown in artificial situations in the laboratory where participants do not know each other, will translate into real settings. However, social capital has also been empirically shown to make a difference in real behaviour related to conservation and agriculture, in a variety of settings. In India, village-level social capital has been found to affect individual contributions towards soil and water conservation activities (Bouma et al. 2008), and, in combination with leadership, is a key factor in the overall success of these activities and other development initiatives (Krishna 2001). Elements of social capital including membership in associations, trust, and community ties are also found to be positively related to the adoption of soil conservation measures in Kenya (Nyanena 2006). In Guatemala, social capital has been shown to improve collective forest management (Katz 2000). Weak social capital, embodied in a lack of cooperation and trust, is identified as a factor behind the failure of some forestry management payments for ecosystem services projects in China (Gong et al. 2010). The willingness of landowners to participate in a Chinese forest restoration program is found to significantly depend

on the percentage of neighbours also participating in the program (Chen et al. 2009), indicating the importance of social norms. Social norms also affect consumer choices regarding the purchase of "green" electricity in Sweden (Ek and Soderholm 2008).

Social capital is also an important factor in collective action to manage and improve environmental resources, such as watersheds or forests. Collective action depends not only on the willingness of group members to trust, cooperate, and reciprocate the behaviour of others, but also on the existence of social norms that provide opportunities for the use of social sanctions to promote behaviour that benefits the group (Ostrom 2000). Programs that focus on building social capital to enable this type of collective action are becoming widespread and proving successful (Pretty and Ward 2001, Pretty and Smith 2004).

Thus, social capital has been shown to make a difference to individual decisions in laboratory experiments, and to be relevant in a variety of real conservation-related activities. The question then becomes whether and how social capital may be expected to affect landowner behaviour in conservation auctions.

Linkages to Conservation Auctions

One potential linkage between social capital and behaviour in conservation auctions is the concept of social norms. In one sense, social norms, operationally defined as the average or common behaviour, exist in any community. However, the weight that individuals place on following social norms, and the level of social approval resulting from compliance with the norm, is likely to be affected by the level of social capital in the community. If individuals have little interaction or exchange with their neighbours, there is little scope for others to observe, care about, or respond to their actions. If, on the other hand, social networks are strong and individuals frequently interact with their neighbours, there are more opportunities for their actions to be scrutinized and for social approval or disapproval to be expressed. Thus, when a high level of social capital exists in a community, social norms regarding actions that affect

environmental quality, including the adoption of BMPs, may play an important role in affecting the behaviour of individuals.

If the social norm supports the adoption of BMPs that improve environmental quality, abiding by the norm will earn social approval for the landowner. Thus, adopting these practices should increase a landowner's utility, separately from any payments received through the program. This may reduce the net opportunity cost of adoption, relative to a situation where such a norm does not exist. Latacz-Lohmann and van der Hamsvoort's (1997) model shows that reducing the net opportunity cost should tend to lower the optimal bid level. Thus, in contexts where positive social norms exist, and where strong social capital causes a high weight to be placed on the norm in an individual's utility function, landowners should tend to submit lower bids than in contexts where these norms do not exist or where social capital is weak. Of course, if the norm is negative towards the adoption of the new practices, it will have the opposite effect.

This impact of social norms on auction outcomes can be shown theoretically by building on the framework developed by Lactacz-Lohmann and Van der Hamsvoort (1997) and described in Chapter 2. As noted above, social norms can be modelled by including an extra term in the utility function that represents the social approval resulting from following the norm. Levitt and List (2007), for example, suggest an additively separable utility function which includes a wealth component and a moral component. The moral component includes the action a, the social norm n, which takes a higher value when the norm is stronger (average behaviour is strongly towards one end of the spectrum), and the level of scrutiny s, which here is assumed to be positively related to the level of social capital. This moral component to utility can be included in the model of Lactacz-Lohmann and Van der Hamsvoort (1997). Suppose that the moral utility function is such that utility is positive when undertaking the BMP if the norm supports this, and negative if the norm does not. Then a farmer will submit a bid b if:

$$[U(\pi_1 + b) + U(a,s,n)]Pr(b \leq \beta) + U(\pi_0)[1-Pr(b \leq \beta)] > U(\pi_0) \qquad (5)$$

Substituting in the certainty equivalent and expectations about β as described in the previous chapter, and rearranging, this gives the following condition:

$$\{[\pi_1 + b - RP_1(b) + U(a,s,n)] - (\pi_0 - RP_0)\}[1 - F(b)] > 0 \quad (6)$$

Maximizing the left-hand side of this equation with respect to b, given the assumptions described previously, gives the optimal bid function:

$$b^* = \max \{\pi_0 - \pi_1 - [RP_0 - RP_1(b)] + [1 - \partial RP_1(b)/\partial b][(1 - F(b))/f(b)] - U(a,s,n), \beta_{min}) \quad (7)$$

$$\text{s.t. } \pi_1 + b^* - RP_1(b^*) + U(a,s,n) > \pi_0 - RP_0$$

Thus, this model shows that social norms regarding the adoption of BMPs may affect both the optimal bid level and the likelihood of submitting a bid at all,[3] since the change to the constraint affects the likelihood of finding a bid level b* that satisfies the constraint. When the norm is positive towards the conservation activities, the optimal bid will be lower the stronger the norm and/or the social capital level, and the likelihood of submitting a bid will be higher. The opposite effects will occur if the norm is negative towards the conservation activities.

Social norms may also play a role in auctions where landowner payoffs from participating in the auction depend on the actions of others. For example, in the group payment auction of Taylor et al (2004), landowners first submit individual bids, and a "team" of successful bidders is chosen by the contractor. Each team member then decides on their actual adoption of new BMPs. If the team as a whole achieves a set environmental target, all team members receive their bid amount. Thus, there may be social pressure on landowners to adopt the BMPs, since their choices in this regard affect the payoffs received by others on the team. However, since this model separates the bid decision from the decision about actual adoption of BMPs, the theoretical model is more difficult to determine.

Another auction format where this could be relevant is one designed to increase participation of landowners. Pilot studies of conservation auctions often

[3] This implies that social networks and norms should be considered as potential factors behind the perceived reluctance of landowners to participate in conservation auctions (Whitten et al. 2007).

suffer from low participation rates, for a variety of reasons (Whitten et al. 2007). To increase participation, an auction could be designed where *all* farmers in a region, community or group receive a lump sum payment based solely upon the number of farmers who submit bids. Farmers whose costs of undertaking the BMPs are too high to participate in the auction could still play a role by putting social pressure on others to submit bids. This auction design would not affect optimal bid levels but would affect the constraint that determines whether or not a bid would be placed; if the increase in the lump sum payment to a farmer due to his own bid submission is g, and the utility from social approval resulting from submitting a bid is U(a,s,n), the constraint would become:

$$\pi_1 + b^* - RP_1(b^*) + g + U(a,s,n) > \pi_0 - RP_0 \quad (8)$$

Thus, including these components makes it more likely that the participation constraint will be satisfied, meaning that more landowners are likely to submit bids.

A second link between social capital and conservation auctions has to do with reciprocity. This is particularly relevant in auctions where landowner payoffs depend on the actions of others, such as in the group auctions described above. Landowners who feel that their neighbours are behaving unkindly by shirking their contribution to the environmental outcome, or failing to submit a bid when payments depend on participation rates, may respond in the same way. According to the theory of reciprocity, a landowner who perceives himself to be treated unkindly by others will receive positive utility from treating others unkindly, but negative utility from behaving in the opposite way. Reciprocity can be included in the theoretical model similarly to social capital, by including an additional utility term either in the bid function or in its constraint. The end result would be a bid function similar to equation (7) above, with a utility term representing reciprocity in place of the term representing social norms.

It is possible that social capital will also promote an undesirable type of strategic behaviour in a conservation auction, collusion. Higher social capital levels may allow increased communication and cooperation among individuals. Landowners may use this ability to agree to increase their bid levels. However,

because this type of behaviour is also likely to require a strong leader to coordinate the group, this possibility will be discussed further in the following chapter, on leadership.

Experimental Design

This section describes a set of experiments that allow the potential impacts of social capital on conservation auctions, as described above, to be tested in the laboratory. The experiments were simultaneously used to examine the impacts of leadership, which will be further discussed in the following chapter. Such simulated auctions are often used to initially test design features of conservation auctions, since real field experiments are expensive and time-consuming. Experimental auctions are a low-cost way to design prototype auctions which can then be implemented as pilot studies in a field context.

However, laboratory experiments pose a difficulty in terms of testing the impact of social capital and other social factors. In the field, auctions take place in real communities where landowners have real social connections. In simulated auctions in the laboratory, however, concerns about participant confidentiality and the prevention of confounding factors mean that experimenters often eliminate any kind of social interaction from the experiment. Participants often have no existing social connections, are not allowed to communicate, and are unable to observe or respond to the behaviour of others. Thus, social factors are essentially unable to play any role in affecting participant behaviour. The challenge of this experiment is to recreate channels and mechanisms through which these factors are able to play a role. On the other hand, the advantage of such simulated experiments is that social interactions can be carefully observed.

Auction Structure

In a simulated conservation auction (e.g. Cason and Gangadharan 2005, Schilizzi and Latacz-Lohmann 2007, Rolfe et al. 2009), participants are presented with individual farm data and told that they represent landowners who must make decisions about how to manage their farm. They are invited to submit bids using a

computer program. The program then ranks the bids according to the auction criteria, and informs participants whether their bid was successful or not. Participants generally earn a small, fixed amount as their "farm profit" and additional earnings (or losses) depend on their bids and adoption costs, within the structure of the auction. For the sake of cost-effectiveness, multiple independent auctions are usually conducted with each group of participants.

The experiments used for this study were implemented similarly to other conservation auction experiments. However, the experiments were not framed as "conservation auctions" and the language of "farm" and "landowner" was not used. Instead, participants were told that they represented "business owners" and that they needed to make decisions about managing their business (for full experimental instructions, see Appendix B). BMP adoption was framed as a "contribution to a social cause that benefits your community." These alterations to standard procedure and framing were made because social capital and environmental preferences have been found to be correlated (Torgler and Garcia-Valinas 2007, Jones et al. 2009). To isolate the effect of social capital on behaviour in these auctions, references to the environment and environmental benefits had to be removed.

The experiment was programmed using Z-Tree (Fischbacher 2007), software designed for conducting economic experiments. Each participant was seated at an individual computer and all relevant information was displayed on the computer screen. All decisions were made anonymously using the computer. Participants were allowed to discuss their decisions and outcomes with others if they wished, but were told that they were under no obligation to do so.

In each auction round, participants were told their contribution cost and the "social benefit" resulting from their contribution. This information is not always revealed in conservation auctions for two reasons: for many BMPs, farmers do not know the exact environmental benefit of adoption, and revealing this information has been found to increase rent-seeking (Cason et al. 2003). However, in this case, adoption resulted in real monetary donations being made to a local charity (for reasons that will be discussed later). Thus, it was necessary to

give participants some idea of the benefit of their contribution. This situation is relevant to some kinds of BMP adoption, such as restoration of wetlands or riparian zones where the farmer knows the area that is being restored. The benefits and costs for this experiment were, in fact, drawn from real data on wetlands restoration by Manitoban farmers (Boxall et al. 2009). They were scaled to an appropriate level for the experiment. Both costs and benefits were heterogeneous across participants.

Each participant was invited to submit a bid, which specified their desired payment in return for their contribution to the social cause. After all bids had been submitted, they were evaluated and ranked based on cost-effectiveness (dollars per unit of social benefit) and the best bids were accepted up to a fixed budget cap, which was unknown to participants. Each bidder was informed of the outcome of his or her bid. Successful bidders were constrained to make the specified contribution to the social cause; unsuccessful bidders were given an opportunity to make an uncompensated contribution to the social cause, after which each participant was shown their profits and outcomes for that round.

The auction used a discriminatory pricing mechanism, where each successful bidder receives their bid amount. Although uniform pricing mechanisms provide an incentive for truthful cost revelation since the dominant strategy is to bid at the cost level, discriminatory pricing is more commonly used in conservation auctions (Latacz-Lohmann and Schilizzi 2005). As well, discriminatory pricing is usually easier for participants to understand, since they simply receive the amount that they asked for. Thus, to keep the auction design as simple as possible, a discriminatory pricing mechanism was chosen.

For each auction, the payments were calculated as follows. Each participant began with a fixed payment of $15 as their “business profit.” Successful bidders received their bid amount in addition to this, and their contribution cost was subtracted from the total. If unsuccessful bidders chose to contribute, their contribution cost was subtracted from the $15 business profit.

Each group of 10 participants participated in a series of 15 independent auction rounds, plus one practice round at the beginning. To prevent wealth

effects from compromising the independence of rounds, participants were informed at the outset that only one round would be binding, resulting in real payments and social benefits. This round was chosen randomly using a rolled die, at the end of the experiment.

The set of costs and benefits were redistributed among participants after rounds 5 and 10. Thus, each participant saw three separate sets of costs and benefits, although the complete set of costs and benefits remained constant throughout the experiment. This was done to prevent boredom among participants, which could have caused them to begin making irrational decisions to liven things up. It is also fairer to participants, since some sets of costs and benefits made it impossible to submit a successful and profitable bid.

Allowing for Interdependence

For social factors to play a role in influencing behaviour, it is necessary to have some mechanism through which individual decisions have some effect on other participants - some reason why participants should care about what others do, not only about their own individual payoffs. In a real auction, this happens naturally because the real environmental quality resulting from BMP adoption has real effects on landowners and others. In an experimental setting, this mechanism is missing.

Two changes to the standard auction design were used to recreate this mechanism. First, in all experiments, contributions made by the participants during the auction resulted in real monetary donations to a local charity, the Campus Food Bank at the University of Alberta. The monetary donations were equal to the social benefit for each participant who either submitted a successful bid or chose to contribute despite an unsuccessful bid. Donations were made for the randomly chosen binding round. Thus, the auction resulted in real social benefits to the participants' community, which mirrors the environmental benefits to society that results from BMP adoption. This is referred to as the "Donations" treatment in the following analysis.

Second, in half of the experimental sessions, an additional group payment was offered on top of the individual bid payments. The donations mechanism was still in place for these sessions. However, participants were also told that if the auction achieved a target amount of contributions, each participant (regardless of whether or not they had submitted a successful bid) would receive an extra $1 payment. The target, which was revealed to participants, was set at the maximum social benefit that could be achieved within the auction budget if there was no rent-seeking behaviour by participants. This mechanism is similar to the gain-sharing payment scheme described by Helper and Kleiner (2010), which they show empirically to improve profitability for a manufacturing plant. Other forms of group incentives have also been used for conservation programs (Maille and Collins 2007, Collins and Maille 2011). For this treatment, the social benefits of contributions were scaled down so that several participants found themselves with costs greater than the social benefit of contribution. The idea is that these participants, who are unlikely to compete successfully in the auction, may put social pressure on other participants to submit low bids and thus earn the group payment for all participants. This treatment is referred to as the "Group Payments" treatment.

Social Mechanisms

To allow social capital (and leadership) to affect individual behaviour, three additional features were introduced into the auction design. These features allow for a closer parallel to real conservation auctions that are carried out in a community of landowners, where the participants are part of existing social networks and are likely to talk to each other about the auction and their own decisions.

First, some information about the decisions made by other participants was revealed during the experiment. In most experiments of this type, participants only receive information about their own adoption costs and the rules of the auction, but are not given any information about the decisions made by others. However, social norms and reciprocity can only play a role when participants

have some information about these decisions. In a real auction, it is quite possible that landowners will talk to others in the community about their participation in the auction program, and that they will observe each other's adoption of BMPs (for example, restoration of wetlands or riparian zones results in visible changes to the landscape). Thus, after each round in the experiment, all participants were told who had submitted a bid, and who had contributed to the social cause. This was intended to give some idea of "average" behaviour which can be used to determine social norms, and provides the information needed for social approval and reciprocity to play a role. To protect confidentiality, this information was revealed by participant number only and participants were not told each other's number.

Second, participants were able to communicate throughout the experiment. This provided an opportunity for social approval or disapproval to be expressed, and for participants (e.g. leaders) to attempt to influence the behaviour of others. Again, this reflects the fact that landowners may communicate with each other during the course of a real auction. Communication was facilitated in two separate ways. Participants were able to chat electronically with the whole group through the Z-Tree software. This allowed participants to comment directly on the actions undertaken by other participants, since each person was identified in the chat by their participant number. Participants were also allowed to verbally communicate at any point during the experiment. This allowed communication to happen in a more natural way.

Third, as in many conservation auction experiments, multiple auction rounds were carried out with each group of participants. This repetition allowed individuals to use and be influenced by information gained from the previous rounds regarding the decisions made by others. This is necessary for the formation of social norms, expression of social approval/disapproval, and reciprocity, because of the fact that all participants submit their bids at the same time in any given auction round. In a real auction, multiple auction rounds may or may not be used. However, even in a single-shot auction, landowners have weeks or months to develop their bid submission, during which time they may communicate with

each other and gather information about each other's actions and intentions. They may also have prior information regarding each other's management practices. The multiple auction rounds in the experiment provide participants, most of whom did not know each other, to gain some of this kind of information.

Social Capital and Leadership Treatments

Within this basic auction design, which remained the same for all sessions, the effects of social capital, as well as leadership, were isolated by varying the composition of groups participating in each experimental session. To do this, prior to signing up for specific experimental sessions, potential participants were invited to fill out a questionnaire designed to measure individual-level social capital and leadership abilities. The questionnaire was administered online, through a secure survey server.

Social capital was measured using questions from the Social Capital Questionnaire (SCQ) designed by Bullen and Onyx (1998), which includes questions based on the theoretical components of social capital.[4] Economic studies have found significant correlations between answers on questionnaires and individual behaviour related to social capital (Glaeser et al. 2000, Anderson et al. 2004, Karlan 2005). The questions are answered using Likert-type scales which range from one (indicating low social capital) to four (indicating high social capital). Therefore, each individual's total score on the questionnaire, adding points for each question, can be used as a numerical summary measure of individual-level social capital. Scores on the survey could potentially range from 29 to 116; scores in our sample ranged from 53 to 111, with an average score of 81.12 and a standard deviation of 11.38. The SCQ questions, with their average scores and standard deviations, are found in Appendix A.

On the basis of their individual scores on the pre-testing questionnaire, participants were invited to register for specific experimental sessions which created four experimental treatments, illustrated in Table 1.

[4] The original survey includes 31 questions. We deleted two questions because of issues raised during the project's ethics review.

Table 1: Experimental Treatments

	Leadership	
Social Capital	high, high	high, low
	low, high	low, low

The high social capital treatments consisted of individuals with relatively high scores on the SCQ (78 or above), while the low social capital treatments consisted of individuals with relatively low scores (under 78).[5] The high leadership treatments included at least one individual identified as a strong leader; further details on the leadership survey are in Chapter 4.

The results of the pre-testing questionnaire showed that social capital and leadership are correlated, at least as measured in these surveys (the correlation coefficient is 0.54). Very few individuals had high leadership scores and low social capital scores. Therefore, the division of individuals into the high- and low-social capital treatments was not exact; some low social capital, high leadership sessions included leaders with high social capital scores. However, the majority of the other participants in these sessions had relatively low social capital scores. To control for this variation, the individual-level data on social capital allows average social capital to be used as the explanatory variable instead of simply using a dummy variable for the high social capital treatment.

An additional experimental treatment consisted of participants drawn from existing social groups (student clubs). This treatment allowed for the alternate conceptualization of social capital as a collective or group-level attribute. The existing social groups, made of participants who already knew each other and had formed relationships, were presumed to have relatively high levels of both collective social capital and leadership, compared to the treatments in which participants did not know each other. Participants in the existing groups were also

[5] The cutoff of 78 was the median score after the first several pre-testing surveys were completed; the median score increased slightly (to 80) after more data was collected.

asked to complete the social capital and leadership questionnaire, to allow these factors to be controlled for in the data analysis.

Participants

The experiment was carried out with student participants at the University of Alberta. The Department of Resource Economics and Environmental Sociology maintains a database of students and other individuals who have signed up to participate in economic experiments. Individuals in this database were invited to complete the pre-screening social capital and leadership questionnaire, and those who completed the questionnaire were then invited to participate in the experiment. For the treatment consisting of a pre-existing group, several student clubs at the University of Alberta were invited to participate. In total, 28 experimental sessions took place with 10 participants in each, making a total of 280 participants. Table 2 shows summary statistics for each of the main experimental treatments.[6]

[6] Due to difficulty in recruiting participants for some of the sessions, the treatments are not completely balanced. However, as we will see below, this does not matter in the analysis, since the original treatments are not used as explanatory variables in the final econometric models. It may be helpful to think of the assigned treatments as a source of variation in social characteristics and interactions, rather than strictly separate conditions to be directly compared to each other.

Table 2: Participant Summary Statistics, by Treatment

Variable	Low Social Capital, Low Leadership	Low Social Capital, High Leadership	High Social Capital, Low Leadership	High Social Capital, High Leadership	Existing Group
# of "Donations" sessions	3	3	3	3	3
# of "Group Payments" sessions	2	3	2	4	2
Percentage of male participants	36%	48%	41%	56%	72%
Average age of participants	24	23	23	24	22
Average social capital score	72	77	86	88	82
Participant had previously participated in an economic experiment	68%	69%	70%	76%	5%
Participant believes the Campus Food Bank is worth donating to	100%	97%	91%	94%	92%

Experiment Results and Analysis

Analysis of the data collected in these experiments happens in two parts. First, analysis of individual bid functions provides insight on the specific mechanisms through which social capital affects individual bidder behaviour. Second, analysis of aggregate auction outcomes, such as cost-effectiveness, shows how this individual behaviour impacts commonly used measures of auction performance.

Bid Functions Analysis

The analysis of individual bid functions is based on the model of optimal bidder behaviour (Latacz-Lohmann and van der Hamsvoort 1997). The dependent variable in a bid function is the non-zero bid[7] submitted by an individual participant in an auction. Based on the bid function presented in Equation 7 (pg. 20), explanatory variables include the contribution cost, along with variables

[7] A model was also run which included individuals who did not submit bids but did adopt BMPs, with the assumption that this, in effect, represents a bid of $0. However, the inclusion of these individuals made very little difference to the coefficients or their significance.

related to social capital, as discussed in previous sections. These regressions also include variables representing the effect of leadership; observations relating to these variables will be discussed in Chapter 4.

Because of the panel structure of the data, any unobserved individual-specific factors that do not change over the course of the experiment, such as risk attitudes or previous experience in similar studies, can be accounted for via individual-specific effects in either a fixed or random effects model. However, random effects regressions are only appropriate when the unobserved individual-specific effects are uncorrelated with the explanatory variables in the regression. In this case, it is likely that this assumption will be violated; for example, individual social capital scores are likely to be correlated with the average social capital score for a group, and social capital may be correlated with other individual-specific characteristics as well. Therefore, this situation calls for a fixed effects regression.[8] This requires some creativity in defining explanatory variables that change over the course of the experiment, since variables that do not change cannot be estimated in a fixed effects model.

Table 3 presents the results of such a fixed effects regression. The dependent variable is individual bid amounts.[9] These are regressed on individual contribution costs (UNITCOST) and on variables representing the experimental treatments imposed in the auctions. UNITCOST is estimable because costs were redistributed among participants every five rounds.

[8] Unfortunately, the use of fixed effects regression does limit our ability to test and correct for problems of heteroskedasticity and autocorrelation in this context. Stata's test for heteroskedasticity in a panel, fixed effects regression (xttest3) is unreliable when N is large and T is small, as in this case (Baum 2001). Furthermore, Stata's robust (clustered) standard errors for fixed effects estimation are unreliable when the number of clusters is less than 50 (Nichols and Schaffer 2007), which applies to this case since clustering of errors could occur on the session level as well as the individual level. However, the potential inefficiency caused by these problems is less of a concern than the inconsistency that would result from incorrectly using a random effects model in this context.

[9] Throughout the analysis, bids submitted during the practice period as well as periods 1, 6, and 11 are excluded. Since the auction format was new to many participants, it took some practice before they were confident in submitting bids, and initial bids involved some trial and error. As costs and benefits were rotated among participants after rounds 5 and 10, rounds 6 and 11 also required participants to adjust to a new set of numbers. Excluding these rounds reduces the noise in the data, while having little effect on the estimated coefficients.

Social capital is represented in this model through the variables SCADOPT and GADOPT. SCADOPT is the interaction between the average social capital score in the group and the lag of ADOPT, a variable indicating the number of participants adopting BMPs in the last period. This formulation is intended to capture the effect of social norms, which, as previously discussed, are closely related to social capital. In the economics literature, social norms are often defined as the frequency of a behaviour in a group, meaning that the number of participants adopting BMPs represents a social norm regarding BMP adoption. If social capital is expected to influence behaviour by strengthening the effect of social norms, the interaction between average social capital scores and the social norm should capture this effect. GADOPT follows the same logic, but for group-level (collective) social capital rather than the average of individual social capital scores. This variable interacts GROUP, a dummy equalling one if the experiment took place with an existing student group, with ADOPT as defined above.

LAGLEAD and GLEAD represent leadership variables, to be discussed in the next chapter. Finally, LNPER is the log of the period number; this variable attempts to control for learning effects that may happen over time as participants grow more comfortable with the experimental procedures. There are 2910 observations in this dataset, from 270 participants.[10]

[10] This analysis excludes one of the existing group sessions, from the "Donations" treatment. Prior to this session taking place, the subjects had agreed to pool their earnings and donate them to their student club. They successfully colluded throughout the experiment, with the expressed goal of maximizing their group earnings as well as contributions to the food bank. Their strategy was for the four participants with the best cost-benefit ratios to capture the entire auction budget (which they discovered through trial and error at the beginning of the auction), with all other participants sitting out of the auction and collecting their $15 profit. Thus, while these results are still relevant for auction outcomes, the bids do not represent individual utility maximization and are therefore excluded from the bid analysis.

Table 3: Effect of Social Capital & Leadership on Bid Functions

Dependent variable: Individual bid amount

Variable	Coefficient (Standard Error)
UNITCOST	0.802*** (0.0765)
LAGLEAD	3.34 (3.10)
GLEAD	19.355** (8.08)
SCADOPT	0.00847 (0.00941)
GADOPT	-6.28*** (2.25)
LNPER	2.13 (1.50)
CONSTANT	-0.519 (5.69)
sigma_u[a]	23.4
sigma_e[b]	48.6
rho[c]	0.187
R^2 within	0.0464

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

[a] Panel-level standard deviation

[b] Standard deviation of ε_{it}

[c] Fraction of variance due to individual effect u_i

The coefficient on UNITCOST in this model is positive, highly significant and close to 1 in magnitude. This indicates that participants are generally behaving as expected in a conservation auction; they are increasing their bids in accordance with their contribution costs, nearly dollar for dollar. The fact that bids are not perfectly correlated with contribution costs is unsurprising and can be explained by risk preferences, expectations about the bid cap, and perhaps by utility associated with social factors that are not captured in the other explanatory variables.

This regression produces some very interesting results with respect to the impacts of social capital on bidding behaviour. In particular, social capital and social norms have no statistically significant impact on bidding behaviour in groups of people that do not know each other (SCADOPT); in existing groups, however, social norms appear to have a strong impact on bidding behaviour

(GLEAD). When a group has existing social ties, higher adoption in a previous period is associated with lower bids in the following period. This implies that participants see that others are adopting and respond by lowering their bids to make it more likely that they can receive compensation and adopt the BMP as well. Thus, social capital (via social norms) appears to have an effect on bidder behaviour, *only when participants are part of an existing social network.*

This finding supports the conceptualization of social capital as a group-level attribute, rather than an individual-level attribute, at least in this context. It may also imply that the use of individual surveys to measure social capital is inappropriate in this context, since this method assumes that individuals will transfer the characteristics and behaviours captured in the survey to a very different social situation (but see Anderson et al. 2004 for evidence in support of this approach). The trust games used to measure social capital in some other studies (Glaeser et al. 2000, Bouma et al. 2008) may present an alternative method to use in future research.

Auction Outcomes Analysis

The analysis of bid functions has shown that social capital, in the collective sense, has a significant impact on individual bidding behaviour. However, the question that policy-makers are most interested in is how social capital might impact overall auction outcomes.

There are several ways to measure auction success. Three key performance measures are described by Cason and Gangadharan (2005). The first is the level of environmental benefit achieved, measured as a percentage of the maximum benefit that could be achieved given the auction budget and landowner adoption costs, assuming that landowners receive exactly their adoption costs as compensation (abbreviated as PMAR, or Percentage of Maximum Abatement Realized). Another is cost-effectiveness, the benefit achieved per dollar, again measured as a percentage of the benefit per dollar achieved in the optimal outcome (POCER, or Percentage of Optimal Cost Effectiveness Realized). A third measure of success is the percentage of total payment which is captured as rent or

profit by the landowners (PRENT). This measures the amount of over-payment by the government relative to actual adoption costs. The percentage of landowners who are willing to submit bids, or who achieve successful bids, may also prove to be an important measure of auction success in some cases. In experimental settings such as this one, however, non-participation in the auction is not usually an issue.

To begin the analysis, Table 4 shows the average of each outcome over all auction periods (excluding the practice period and periods 1, 6 and 11), by experimental treatment. This table also separates the data by the type of payment mechanism used. Recall that in the "Donation" treatment, adoption simply resulted in donations to the campus food bank. In the "Group Payments" treatment, adoption resulted in donations to the food bank *and* participants received a bonus payment if the contribution target was reached.

Table 4: Average Auction Outcomes, by Treatment

(Average values; standard deviations in parentheses)

Variable	Low Social Capital, Low Leadership	Low Social Capital, High Leadership	High Social Capital, Low Leadership	High Social Capital, High Leadership	Existing Group
			Donation Treatment		
Number of observations	36	34	36	36	35
PMAR	105% (32)	103% (29)	105% (25)	100% (26)	97% (34)
POCER	285% (66)	280% (57)	265% (94)	276% (63)	258% (94)
PRENT	-45% (37)	-45% (33)	-36% (53)	-40% (35)	-32% (53)
			Group Payments Treatment		
Number of observations	24	36	24	48	24
PMAR	84% (22)	93% (32)	111% (27)	92% (23)	109% (38)
POCER	123% (32)	139% (39)	135% (30)	128% (25)	140% (34)
PRENT	-16% (29)	-29% (42)	-28% (30)	-14% (24)	-34% (34)

A few observations are immediately apparent. First, many of these outcomes appear to be higher than the optimal outcomes. For the donation treatment, PMAR is above 100% for almost all the treatments; POCER is very high for the donation treatment and still higher than 100% for the group payments treatment. In all treatments, PRENT is negative, meaning that on average, participants were not fully compensated for their contribution costs in the auction. In all these cases, the explanation lies in the fact that many participants in the experiments submitted bids that were below their contribution costs. Thus, they made a loss in the auction and had to use part of their $15 "business profit" to cover their social contributions. Some participants also donated even if their bids were unsuccessful, paying for the contributions out of their business profit.

While this would be a highly unusual finding in most conservation auction experiments, it can be explained by the fact that in this experiment, BMP adoption resulted in real monetary donations to the campus food bank. This is a popular charity among participants, 95% of whom said yes to the question "Do you think the U of A Campus Food Bank is a charity worth donating to?" Thus, it seems that participants who gained utility from donating to the food bank (whether from altruism, social norms, reciprocity or other factors) were willing to take a small monetary loss in order to make this contribution. Of course, this is something that may be observed in the real world as well.

However, part of the explanation may also lie in participants' failure to immediately grasp the rules of the auction. Several participants initially expressed confusion about how much to bid and stated that they were using trial and error to come up with their bids. As they became more comfortable with the auction procedures, most participants realized that in order to make a profit, their bids would need to be at least high enough to cover their contribution costs. This learning experience may help to explain the difference between the Donation and Group Payments treatments. Because there was a limited pool of participants for this experiment, many individuals participated in both treatments, with the Group Payments treatment coming after the Donation treatment. After the experiment, participants were asked whether they had ever participated in an economic

experiment before; 45% of participants in the Donation treatment had, while 75% of participants in the Group Payments treatment had. Correspondingly, the auction outcomes in the Group Payments treatment are not as unusually high as in the Donations treatment. This is also supported by the fact that the decline in auction outcomes is not seen for the experiments conducted with existing groups, where very few participants had previously participated in an economic experiment. Fortunately, the panel structure of the data allows us to control for this difference in previous experience.

A note of caution is needed when comparing POCER and PRENT across the Donations and Group Payments treatments. As previously explained, participants in the Group Payments treatment each received a bonus payment of $1 if the group contribution target was reached. This bonus payment is included in the calculation of the optimal outcomes; that is, at the optimal level of adoption, the contribution target is reached and the bonus payment is therefore taken into account when determining the cost-effectiveness of this optimal outcome. However, if a group fell just short of the target, they would not receive the group payment. This would result in a minimal reduction in environmental benefit, with a relatively large ($10) reduction in total payments. The cost-effectiveness of such an auction could easily be an improvement on the "optimal" outcome. The bonus payments also increase the amount of rent that participants are able to capture.

Other than these observations, these summary statistics do not show any obvious patterns in terms of the effect of the various experimental treatments on auction outcomes. To investigate this, further analysis is needed.

Table 5 presents the results of fixed effects[11] models regressing each auction outcome on the explanatory variables used in the bid functions (omitting UNITCOST, which is meaningless in these models). In this case, the panel data has session-specific effects instead of individual-specific effects. There are 334 observations for the PMAR model and 333 observations for the POCER and PRENT models (due to one session where all bidders were unsuccessful, making

[11] Fixed effects estimation is used for the same reasons explained above; it is also based on a Hausman test that rejects the null hypothesis that the random effects model using these variables is consistent, for both the PMAR and PRENT models.

it impossible to calculate POCER and PRENT), from 28 sessions. As before, the practice period and periods 1, 6 and 11 are excluded.

Table 5: Effect of Social Capital & Leadership on Auction Outcomes

Variable	Coefficient (Standard Error)		
	PMAR	POCER	PRENT
LAGLEAD	-9.23* (5.06)	-19.1* (10.0)	11.3* (64.8)
GLEAD	-3.61 (12.5)	31.6 (24.8)	-4.44 (16.0)
SCADOPT	-0.0159 (0.0151)	-0.0227 (0.0299)	0.0168 (0.0194)
GADOPT	6.63** (3.01)	11.8** (6.00)	-6.00 (38.9)
LNPER	-6.11*** (2.35)	-12.9*** (4.66)	8.66*** (3.02)
CONSTANT	116*** (8.92)	232*** (17.7)	-53.6*** (11.5)
sigma_u[a]	19.3	0.86.2	26.8
sigma_e[b]	26.0	0.51.4	33.3
rho[c]	0.355	0.738	0.394
R^2 within	0.0532	0.0518	0.0461

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

[a] Panel-level standard deviation

[b] Standard deviation of ε_{it}

[c] Fraction of variance due to individual effect u_i

To interpret the results in Table 5, it is important to note the meaning of the signs of the coefficients. For the PMAR and POCER models, which measure environmental benefits and cost-effectiveness, respectively, relative to the optimal outcomes, a positive sign means that auction outcomes are improved. For the PRENT model, which measures the percentage of payments that are captured by landowners as information rent (in a sense, money wasted by the government or other auctioneer), a negative sign indicates improved auction outcomes.

These results are consistent with the results from the bid functions analysis, in terms of the effects of social capital. Outside an existing group, social norms, which had no impact on individual bids, also have no significant impact on auction outcomes (SCADOPT). However, in existing groups, social norms do

appear to improve auction outcomes (GADOPT); this agrees with the finding that social norms cause participants in existing groups to reduce their bids. These results are significant at the 5% confidence level for both PMAR and POCER, and nearly statistically significant (p-value of 0.12) for PRENT. The magnitudes are similar for all three outcomes. Environmental benefits are improved by 7% when one more person adopts the BMP in the previous period, in an existing group; cost-effectiveness is increased by 11%, and rent is reduced by 6%.

It is also worth noting that the coefficients on LNPER indicate a significant decline in auction outcomes over the course of each experimental session. This is likely due to the learning effects described above.

Effect of Group Payments Treatment

In the preceding analysis, the fact that fixed effects regressions are used has made it difficult to determine whether the two different incentive mechanisms used in the experiment, donations and group payments combined with donations, have any effect on bidder behaviour or auction outcomes. Since these treatments remain the same over the course of experiment, including them in the analysis requires a large number of interaction terms. However, since the Group Payments treatment does introduce an incentive that is quite different from the donations mechanism alone, it is important to at least attempt to analyze its impacts on bidder behaviour and auction outcomes.

Table 6 presents the results of a fixed effects regression of individual bids, similar to the bid function analysis shown in Table 4, but with each social capital and leadership variable also interacted with a "Group Payments" dummy variable (GP).[12] Thus, GPADOPT is the interaction of SCADOPT and GP, and GPGADOPT is the interaction of GADOPT and GP, GPLEAD is the interaction of LAGLEAD and GP, and GPGLEAD is the interaction of GLEAD and GP.

[12] This does introduce some collinearity between the interaction variables, which was not present in the initial variables. For example, GPGLEAD and GPGADOPT are highly correlated, simply because there are relatively few observations where they are positive (existing groups in the Group Payments treatment). However, separating the correlated variables by dividing the social capital and leadership variables into separate models makes little difference to the variance of the coefficients, so multicollinearity does not appear to be causing a problem in this case.

Table 6: Effect of Group Payments on Bid Functions

Dependent variable: Individual bid amounts

Variable	Coefficient (Standard Error)
UNITCOST	0.802*** (0.764)
LAGLEAD	8.89** (4.39)
GPLEAD	-11.0* (6.27)
GLEAD	-12.6 (12.4)
GPGLEAD	53.2*** (16.7)
SCADOPT	0.0121 (0.0131)
GPADOPT	-0.465 (1.49)
GADOPT	1.37 (4.11)
GPGADOPT	-9.54* (4.94)
LNPER	1.62 (1.52)
CONSTANT	-1.65 (5.71)
sigma_u[a]	22.7
sigma_e[b]	48.6
rho[c]	0.180
R^2 within	0.0510

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

[a] Panel-level standard deviation

[b] Standard deviation of ε_{it}

[c] Fraction of variance due to individual effect u_i

These results show that while the effect of social norms appears to be the same as in the previous analysis, having a significantly negative impact on bids only in existing groups, this effect is now isolated to existing groups who face the group payment mechanism (GPGADOPT). This implies that social norms are successful in influencing behaviour when individual decisions have a direct impact on the outcomes of others (through the group bonuses), instead of a more distant impact on society in general (through donations to the food bank). Again, the impact also requires that individuals a members of existing social networks, giving support to the conceptualization of social capital as a group-level attribute.

What impact does the group payment mechanism have on auction outcomes? It is important to keep in mind the fact that this mechanism can affect outcomes in two different ways. As shown above, it does have an impact on the decisions made by individual bidders, which is likely to impact overall auction outcomes. However, the bonus payments can also have a direct impact on POCER and PRENT, as explained previously. To untangle these impacts, two separate models are presented in Tables 7 and 8. In Table 8, the standard auction outcomes are regressed on the explanatory variables used in the bid function analysis. This shows the overall impact of the group payments mechanism on the auction outcomes, which is likely the impact that policy-makers are most interested in. In Table 7, however, POCER and PRENT are re-calculated, excluding the bonus payments from both the optimal outcome and from the actual payments given to participants. These results isolate the behavioural impacts of the group payments mechanism, rather than the impacts resulting from the auction structure.

Table 7: Effect of Group Payments on Auction Outcomes, Excluding Bonus Payments

Variable	Coefficient (Standard Error)		
	PMAR	POCER	PRENT
LAGLEAD	-5.28 (7.28)	-39.7*** (14.5)	21.6** (9.51)
GPLEAD	-6.82 (10.2)	41.1** (20.3)	-15.8 (13.3)
GLEAD	4.50 (17.4)	106*** (34.7)	-51.9** (22.8)
GPGLEAD	-10.1 (25.6)	-134*** (50.9)	88.2*** (33.5)
SCADOPT	0.00492 (0.0214)	-0.0181 (0.0425)	0.0800 (0.0279)
GPADOPT	-3.15 (2.38)	-1.36 (4.73)	2.22 (3.11)
GADOPT	3.10 (4.14)	9.76 (8.32)	-3.42 (5.47)
GPGADOPT	2.06 (6.04)	-13.7 (12.1)	4.89 (7.94)
LNPER	-6.63*** (2.38)	-12.6*** (4.74)	8.70*** (3.12)
CONSTANT	117*** (8.93)	250*** (17.8)	-62.9*** (11.7)
sigma_u[a]	18.3	74.5	34.7
sigma_e[b]	26.0	51.6	34.0
rho[c]	0.332	0.675	0.511
R^2 within	0.0516	0.0753	0.0690

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

[a] Panel-level standard deviation

[b] Standard deviation of ε_{it}

[c] Fraction of variance due to individual effect u_i

Unfortunately, the results from these tables do not provide any clear conclusions regarding the impact of social capital on auction outcomes. In Table 7, which isolates the behavioural effects of social capital on auction outcomes, none of the variables related to social norms are statistically significant. Since social capital did have a significant effect in the bid function, it seems that either this effect is not strong enough to make a difference in auction outcomes, or that the increase in the number of variables in this model simply results in a lack of precision.

Table 8: Effect of Group Payments on Overall Auction Outcomes

Variable	Coefficient (Standard Error)		
	PMAR	POCER	PRENT
LAGLEAD	-5.29 (7.28)	-39.9*** (14.2)	21.8** (9.22)
GPLEAD	-6.82 (10.2)	40.9** (19.9)	-20.9 (13.0)
GLEAD	-4.64 (16.9)	73.3** (32.9)	-33.5 (21.4)
GPGLEAD	-0.918 (25.3)	-96.7** (49.2)	67.7** (32.0)
SCADOPT	0.00464 (0.0214)	-0.0186 (0.0417)	0.00832 (0.0271)
GPADOPT	-3.13 (23.8)	-1.06 (4.64)	1.55 (30.1)
GADOPT	7.01* (40.9)	24.2*** (8.05)	-12.0** (5.24)
GPGADOPT	-1.84 (6.01)	-27.7** (11.8)	14.0* (7.65)
LNPER	-6.62*** (2.38)	-11.9** (4.64)	8.25*** (3.02)
CONSTANT	116*** (8.94)	232*** (17.4)	-5.33*** (11.3)
sigma_u[a]	20.9	87.1	39.0
sigma_e[b]	26.0	50.6	32.9
rho[c]	0.392	0.747	0.583
R^2 within	0.0632	0.0914	0.0728

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

[a] Panel-level standard deviation

[b] Standard deviation of ε_{it}

[c] Fraction of variance due to individual effect u_i

In Table 8, which includes both the behavioural and structural effects of the group payments mechanism, the results for social capital are somewhat puzzling. The coefficients on GADOPT and GPGADOPT are statistically significant for both POCER and PRENT in Table 8, whereas they were not in Table 7. It is not clear why this should occur, since if anything the structural impacts of the group payments mechanism should obscure the behavioural effects, not accentuate them. In addition, the sign on GPGADOPT is opposite of what might have been expected. This variable captures the effect of social norms in the context of an existing group with group payments. In the bid function, it was negative, meaning that a social norm in favour of adoption appeared to lower bids

in this context, but here it appears to decrease cost-effectiveness and increase rent. This finding is probably related to the issue raised previously about the structure of the group payments treatment. If participants in the group payments treatment tended to lower their bids when faced with a stronger social norm in favour of contributing, the group would be more likely to achieve the social contribution target. In this case, the group members would receive their bonus payment, potentially decreasing the cost-effectiveness of the auction (depending how much additional social benefit was gained), and increasing the amount of rent captured by participants.

Conclusion

This analysis has shown that social capital can have a significant impact on bidder behaviour and on the outcomes of conservation auctions. In the experiments described here, social norms in favour of BMP adoption are found to reduce bids and improve auction outcomes, in groups that have existing social ties. Social norms have no impact, however, when participants do not know each other, regardless of the average level of individual social capital scores in the group. These findings extend the literature on the importance of social capital to conservation issues, to the particular case of conservation auctions. They also provide support for the collective, rather than individual, conceptualization of social capital.

It is possible to interpret these findings in terms of reciprocity, rather than social norms. Instead of responding to social norms, it may be the case that individuals lower their bids out of a desire to reciprocate, when others are seen to contribute. This, in turn, improves auction outcomes. It is not possible to untangle these two explanations within this experiment; further research may shed light on this issue. Since both social norms and reciprocity are components of social capital, however, the importance of social capital remains clear.

Social capital also interacts, in interesting but somewhat confusing ways, with the specific incentives provided through the experimental design. In particular, the bid function analysis shows that the impact of social norms (or

reciprocity) appears to be isolated to the Group Payments treatment, where individual bidder decisions have a direct impact on the outcomes of others within the group. Interestingly, experimental research on group incentives by Nalbantian and Schotter (1997) also found that social norms established within a group had a significant impact on individual behaviour, within certain group incentive schemes.

The impacts of the group incentive on auction outcomes are unclear. Again, further research on the impacts of these specific incentive mechanisms may prove helpful in solving this puzzle. Nevertheless, the distinction is important for researchers studying the impacts of conservation auctions in the lab, since it introduces another experimental design feature that may influence bidder behaviour. It is also likely to be important in policy terms, since BMPs vary in their impact on landowners. Some benefit both landowners and the rest of society, while others provide little or no benefit directly to landowners. The impact of social capital may therefore vary, depending on the specific BMPs that are funded through a conservation auction.

Chapter 4: The Influence of Leadership on Conservation Auctions

In many communities where conservation auctions may be held, official or unofficial community leaders exist. Leaders influence the behaviour of others in variety of ways. It is possible that community leaders will influence the behaviour of others, either in terms of participation in the auction or in suggesting particular kinds of bidding behaviour. This section, therefore, investigates the effect of leaders in the context of an experimental conservation auction.

Economic Theories of Leadership

In fields other than economics, theories of leadership abound (Bass 2008). In the field of economics, however, literature on leadership is fairly scarce. Three main theories exist: leadership as information signalling, leadership by reciprocity, and leadership as a means of solving coordination problems.

One theoretical explanation for the effect of leaders on follower's behaviour is the idea that leaders may have information that is not available to followers, and use this information to intentionally signal the optimal behaviour to followers. Hermalin (1998) developed this theory, focusing on the behaviour of workers in teams. In this model, the payoff to effort goes to all workers according to a pre-determined sharing rule, giving individuals an incentive to free ride. Thus, this setup is similar to the public goods problem typical of many conservation issues. In Hermalin's theory, the payoff to effort depends on a state parameter. The leader, who shares in the team payoff, is assumed to have information about this parameter which is not known to the rest of the team. Thus, he seeks to influence the effort provided by followers by sending a signal about the value of the parameter. This can happen in three ways: announcing the value of the parameter, leading by example (providing effort), or leading by sacrifice (providing a gift to followers in return for effort). Hermalin shows that leading by example and leading by sacrifice may both be credible signals, and thus influence

follower effort, under certain conditions. Hermalin (2007) also extends this theory to a repeated game, where a leader can develop a reputation for honesty that may overcome the need for costly signalling. A similar model applies to other public goods problems, such as charitable donations (Potters et al. 2001).

A second explanation for the effect of leaders is related to the theories of reciprocity and fairness described above with respect to social capital. Huck and Rey-Biel (2006) provide a formal model of leadership by example where inequality aversion is the motivating factor. They assume that some individuals have conformist preferences; they receive disutility from differences in effort between themselves and another agent. Thus, conformist followers will take the leader's actions into account when choosing their own actions. This model could equally well be framed in terms of reciprocity; agents receive disutility from shirking when leaders work hard, or from working hard when leaders are shirking, with the same effect.

A third possibility is that leaders simply take the first move in a coordination problem, thus providing a way for followers to organize themselves to achieve the highest payoff. This is the model described in Wilson and Rhodes (1997). It does not depend on leaders having any information that is unavailable to followers; a leader can solve the coordination problem simply by taking an action which, if followed by everyone else, would achieve the desired outcome. The success of leadership signals in this model does depend on leaders having the same interests as those of followers; if this is not the case, the signal may not be credible and followers may not obey.

An additional theory of leadership, which lies outside the economic literature, needs to be mentioned here since it plays a role in the experimental design for this study. The economic theories mentioned above focus primarily on the *mechanisms* through which a leader may have an effect on the behaviour of others, instead of on the characteristics of the leader himself. Multifactor leadership theory (Bass 1985), also called transformational leadership theory, focuses instead on the individual characteristics and traits that give rise to certain types of leaders. In this theory, transactional leaders motivate followers to achieve

defined goals by offering rewards or appealing to their self-interest; transformational leaders, who are personally charismatic, inspire followers to transcend their own self-interest for the good of the group; and laissez-faire leaders do not lead. This is "perhaps the most widely cited comprehensive theory of leadership that encompasses a range of leader behaviours" (Tejeda et al. 2001: 31) and – importantly for this study – has given rise to a tool widely used to identify leaders, the Multifactor Leadership Questionnaire. This tool will be discussed later in this chapter. In the meantime, it is important to note that this theory is not necessarily an alternative to the economic theories of leadership presented previously; instead, it may serve as a way to identify individuals who are likely to attempt to motivate others through the mechanisms described above.

Empirical Evidence on Leadership

The limited empirical work on leadership, in the economics literature, mainly uses laboratory games to examine the effects of leadership. Some attention has been paid to distinguishing between the information signalling and reciprocity explanations for the effect of leadership in public goods games. The key to this distinction comes from comparing the behaviour of followers when information is asymmetric and when it is symmetric. When information is asymmetric, differences in follower behaviour may be due to either signalling or reciprocity. When it is symmetric, however, any differences in follower behaviour can be attributed to reciprocity, since there is no role for information signalling in this case. Mixed evidence has been found on this question. While Meidinger and Villeval (2002) find evidence for both reciprocity and information signalling, Potters et al. (2001) find that signalling, not reciprocity, explains behaviour. Potters et al. (2007) find that reciprocal behaviour does exist, but that it does not raise overall contributions or earnings; information signalling does raise contributions and payoffs for participants.

The previous experiments focus on public goods games and use pairs of participants. Moxnes and van der Heijden (2003) provide a twist on these experiments by studying a repeated public bads game, with one leader (who

makes the first move) and a group of followers making simultaneous moves. They find that the presence of a leader, who is constrained to set a good example by contributing only a small amount to the public bad, significantly lowers followers' contributions to the public bad relative to a situation where there is no leader. All participants in this experiment have the same information, so information signalling does not play a role here. The study is unable, however, to determine the exact channel through which leadership has an effect.

A number of studies have addressed the theory that leadership can provide a solution to coordination problems. Wilson and Rhodes (1997) use a lab game where payoffs depend on the number of players choosing the same option. Introducing a leader who can send suggestions to followers as to which option they should choose significantly improves coordination and thus payoffs. Brandts and Cooper (2007) take another approach, with a repeated team game where payoffs depend on the minimum effort provided by any player. Allowing a "manager" to communicate with players and provide financial incentives improves coordination and earnings; interestingly, the effect of communication is much stronger than the effect of financial incentives, and the specific content of communication is important. In a similar study with no explicit manager and no communication, Brandts and Cooper (2006) find that unofficial leaders nevertheless emerge and, by setting an example, are able to draw others into higher effort in repeated rounds of the game. When asymmetric effort costs are introduced (Brandts et al. 2007), the same results are found; furthermore, leaders tend to emerge from among players with the most common cost levels.

Linkages to Conservation Auctions

This overview of the limited economic literature relating to leadership suggests several potential links to conservation auctions. These linkages may be both positive and negative from the standpoint of auction success.

The information signalling theory of leadership may play a role in conservation auctions if information relating to the auction is asymmetric and if the decisions of participants affect not only their own payoffs, but the payoffs

received by others. This could be the case even in an individually-based auction, if some of the environmental benefits resulting from the adoption of BMPs go to landowners. A community leader who has knowledge about these benefits (which may not be known to all landowners) could use this knowledge to persuade or signal other landowners that participation in the auction and adoption of BMPs is in their own interest. Leaders could play a similar role in a group auction where payoffs are directly dependent on the actions of others, if they are better informed about how the auction works and what the payoffs are likely to be.

Information signalling can be incorporated into Latacz-Lohmann and Van der Hamsvoort's (1997) model in a slightly different way than the social capital theories described in the previous chapter. Instead of adding an extra component to utility, information signalling implies that leaders are able to change the perceived net payoff from adoption, $\pi_0 - \pi_1$. Thus, the original bid function from Latacz-Lohmann and Van der Hamsvoort (1997) holds:

$$b^* = \max \{\pi_0 - \pi_1 - [RP_0 - RP_1(b)] + [1 - \partial RP_1(b)/\partial b][(1 - F(b))/f(b)], \beta_{min})$$

$$\text{s.t. } \pi_1 + b^* - RP_1(b^*) > \pi_0 - RP_0$$

Leaders may also affect the outcomes of conservation auctions through reciprocity, in essentially the same way as described in the section on social capital. Community leaders may set an example for other landowners by participating in an auction and adopting BMPs. If others care about reciprocity, they will then gain utility by following the actions of the community leaders. Obviously this could work in a negative way as well; if leaders choose not to participate, followers would then gain disutility from participating. As discussed in the previous chapter on social capital, reciprocity can be modelled by including an extra utility term in the model.

A third possibility is that leaders may be able to solve the coordination problem related to collusion. Depending on the specific auction design, some participants in an auction may be able to increase their payoffs by colluding to raise their bids. Collusion represents a coordination problem because participants must agree to raise their bids together; any cheating by individuals who decide to

offer a lower bid (thus making it more likely that their bid will be accepted) can quickly bring about an end to collusive behaviour and thus to the increased payoffs enjoyed by each participant. A strong leader may be able to encourage participants to form and abide by a collusive agreement. Clearly, from the standpoint of auction success, this type of leadership would have a negative impact on cost-effectiveness, and potentially on the environmental benefits resulting from the auction.

Experimental Design

Leadership was studied using the same set of experiments previously described in the section on social capital (Chapter 3). Because the experiments used a 2x2 design which interacted both main experimental treatments, social capital and leadership, all elements of the experimental design are exactly the same as previously described.

Most experimental studies of leadership in the economic literature create leaders artificially, by randomly choosing one participant in the experiment and providing this participant with different information or choices than those faced by the other participants. Our approach is different and, to our knowledge, unique in the economic literature on leadership. In this study, leaders were created by placing individuals who had already been identified as potential leaders, based on their own characteristics and behaviour, into particular groups. These leaders had the same information and made the same kinds of decisions as everyone else; they were not forced into positions of leadership. However, through the design of the experiment, they were able to use their already existing personal leadership abilities to behave as leaders, if they wished.

The mechanisms which allowed for leaders to use their leadership abilities are the same mechanisms that allowed social capital to play a role in influencing behaviour. These include the donations and group payments incentive mechanisms. These gave leaders a reason to care about the decisions made by others, since these decisions affected both wider society (through donations to the food bank) as well as the members of the group itself (through the group

payments). In addition, the information provided about the decisions made by others gave leaders a means to evaluate the decisions made by others, providing a basis for their own leadership decisions. The ability to communicate with followers is especially important for leaders; the verbal and electronic chat allowed leaders to attempt to influence the behaviour of the other participants throughout the experiment. Finally, the multiple auction rounds allowed leaders to respond to decisions made in previous rounds and to encourage behavioural changes in the future.

To identify potential leaders and place them into the appropriate experimental treatments, leadership characteristics were measured in the pre-screening survey. This was done using the Multifactor Leadership Questionnaire (MLQ) (Avolio and Bass 1995), which identifies the primary leadership style (transformational, transactional, laissez-faire or non-leader) of individuals, as well as numerical scores for several dimensions of leadership behaviour. Because this pre-testing was intended only to identify individuals with characteristics that would make them likely to behave as a leader during the experiment, not to identify the leadership style of each participant, the questionnaire included only those questions from the MLQ that make up the transformational and transactional leadership style scores. A sample of the MLQ questions is included in Appendix A.

Our survey included 28 questions from the MLQ in total, with 20 questions for the transformational leadership style and eight for the transactional leadership style. All questions are answered using a Likert-type scale ranging from zero to four, which allows scores for each individual to be totalled. Thus, potential leadership scores on the survey could range from zero (indicating that the individual is not a leader) to 112 (indicating strong transactional and transformational leadership characteristics). Scores in our sample ranged from 46 to 107, with an average total score of 75.98 and a standard deviation of 12.94. The average score on the transformational leadership questions was 55.65 and the average score on the transactional leadership questions was 20.33.

Each high leadership treatment group included at least one individual identified as a transformational or transactional leader by the MLQ.[13] While the MLQ provides a continuum of scores, aimed not at pigeonholing individuals but at identifying particular leadership characteristics, for the purposes of this experiment it was necessary to identify particular individuals who were likely to behave as leaders. Thus, individuals with high scores on the MLQ questions overall (above 91), or in the transformational (above 70) or transactional (above 27) questions on the questionnaire, were identified as strong leaders.[14] The existing student groups which took part in the experiment were also assumed to already have one or more leaders within their group (an assumption which was confirmed by observed behaviour in the experiment, as will be seen shortly).

Results and Analysis

As in the previous chapter on social capital, the impacts of leadership are studied through bid functions as well as analysis of auction outcomes. While some of the analysis uses the same models estimated in the previous chapter, this chapter also goes into detail on different kinds of leadership that were observed during the experiments.

Measuring Leadership

The key variable for this part of the analysis is *observed leadership*, rather than the high leadership treatment itself. Observed leadership is defined as communication that could potentially influence the behaviour of others. This included, for example, explanation of the experimental procedures and their social implications, suggestions regarding bid amounts, and praise or criticism of individual behaviour or auction outcomes. Verbal communication during the experiment was noted by the researchers, and communication via electronic chat was recorded as part of the experimental data. A dummy variable, LEADER, was

[13] Three "high leadership" individuals were assigned to each session. However, in some sessions, some individuals failed to show up. Each session identified as a high leadership session did have at least one high leadership individual.

[14] These cutoffs were set at levels which would provide a sufficient number of individuals identified as strong leaders for the experimental treatments.

used to code periods during which this type of behaviour was observed (1 = leadership was observed; 0 = no leadership observed).

Observed leadership, rather than the high leadership treatment, is used as the explanatory variable in this analysis for two reasons. First, because fixed effects estimation is used, the impact of the high leadership treatment cannot be estimated since it does not change over the course of an experimental session. Observed leadership, on the other hand, can and does vary from period to period.

Second, and more importantly, the MLQ questionnaire was found to do a poor job of predicting which individuals were likely to actually behave as leaders during the experimental sessions. Table 9 presents the results of a random effects probit model that regresses observed leadership behaviour (LEADER) on the experimental treatments.[15] The data are in panel format with session-specific effects.

Table 9: Effect of Experimental Treatments on Likelihood of Observed Leadership

Dependent variable: Observed leadership (1 = yes, 0 = no)

Variable	Coefficient (Standard Error)
HIGHSC	-1.11** (0.552)
HIGHLEAD	-0.247 (0.557)
GROUP	3.71*** (0.856)
GROUPPAY	-0.194 (0.501)
CONSTANT	-0.467 (0.522)
sigma_u[a]	1.14 (0.243)
rho[b]	0.567 (0.104)

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

[a] Panel-level standard deviation

[b] Fraction of variance due to individual effect u_i

[15] This regression includes data from all periods, including the practice period. There are 446 observations from 28 experimental sessions.

These results show that observed leadership is significantly less likely in high social capital groups, and significantly more likely in existing groups. Once this factor is accounted for, however, the presence or absence of an individual predicted to be a leader (either an individual with a high MLQ score, or a leadership position within an existing group) does not appear to be a significant predictor of actual leadership behaviour in these experiments. This may be because the experimental setting is a very different context from the everyday situations at work, school or home which individuals are asked to consider while answering the MLQ questions. For example, individuals who behave as leaders within a group of people they know well may not necessarily be willing to lead a group of strangers.

However, we are primarily interested in measuring the impact of *actual* leaders on conservation auctions. While pre-screening with the MLQ appears to have been unsuccessful as a means to encourage variation in leadership among experimental treatments (an observation which may call for further research on alternative means of identifying and predicting leaders), leaders did nonetheless emerge in some groups and not in others. This allows us to use observed leadership as a variable in the analysis.

To provide a general overview of observed leadership behaviour in the experiment, Table 10 shows the number of periods in which various types of leadership were observed, separated by the main experimental treatments.[16] LEADER is a dummy variable indicating that leadership behaviour (of any kind) was observed during the period. The remaining variables separate LEADER into specific types of leadership behaviour. Four common themes were identified in the verbal and written communication: explanation, collusion, influence, and food bank reminders. Explanation (EXPLAIN) involves clarification of the rules of the auction, without specific suggestions for behaviour (e.g. explaining that bids are accepted up to the budget limit). Collusion (COLLUDE) involves a suggestion to other participants that they raise their bids; this is an attempt to have individuals work together to capture more rent. Influence (INFLUENCE) involves intentional

[16] This table includes all data, as in the previous regression.

efforts to get other participants to behave in some specific way. This category includes attempts to collude, but also includes other suggestions for behaviour such as aiming for a low ratio between a participant's bid and their social benefit. Food bank reminders (FOODBANK) are verbal reminders to participants about the social benefits resulting from BMP adoption, which in this case are real monetary donations to the campus food bank. These themes are not necessarily mutually exclusive (for example, collusion is a subset of influence), and more than one of them could, and often did, take place in a given auction period.

Table 10: Percentage of Auction Periods in which Leadership was Observed, by Treatment

Variable	Low Social Capital, Low Leadership (n = 80)	Low Social Capital, High Leadership (n = 94)	High Social Capital, Low Leadership (n = 80)	High Social Capital, High Leadership (n = 112)	Existing Group (n = 80)
LEADER	18%	47%	23%	4%	88%
EXPLAIN	9%	24%	8%	4%	29%
COLLUDE	11%	23%	1%	0%	43%
INFLUENCE	13%	41%	18%	0%	76%
FOODBANK	4%	4%	4%	1%	30%

These summary statistics show differences in leadership behaviour between the different treatments. Observed leadership of all types is much more common in existing social groups than in groups that did not know each other (it may also be worth noting that most communication within existing groups was verbal, while the vast majority of communication by other groups was through the electronic chat). Leadership was also observed fairly frequently in low social capital, high leadership groups, but very rarely in high social capital, high leadership groups – a puzzling finding.

One specific type of leadership behaviour of great interest to policy-makers is collusion. If individuals successfully collude in an auction, the amount of information rent captured by participants is likely to increase and the cost-effectiveness and environmental benefits resulting from the auction are likely to

be reduced. While these potential outcomes are considered later in the analysis, it is interesting to see whether the likelihood of *attempted* collusion depends on the experimental treatments studied in this experiment. Table 11 presents the results of a random effects probit model that regresses COLLUDE (observed encouragement to others to raise their bids) on the experimental treatment variables, again using all data.

Table 11: Effect of Experimental Treatments on Likelihood of Attempted Collusion

Dependent Variable: Observed attempts to collude (1 = yes, 0 = no)

Variable	Coefficient (Standard Error)
HIGHSC	-0.100 (0.0627)
HIGHLEAD	0.775 (0.829)
GROUP	1.90** (0.902)
GROUPPAY	-1.51** (0.727)
CONSTANT	5.55 (4.87)
sigma_u[a]	1.37 (0.393)
rho[b]	0.654 (0.129)

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

[a] Panel-level standard deviation

[b] Fraction of variance due to individual effect u_i

As before, groups that were predicted to have strong leaders based on the MLQ are no more likely to attempt to collude than other groups. Attempted collusion is significantly more likely to occur in existing groups. However, this regression also shows that the group payment mechanism, where participants receive a bonus payment if the group meets its social contribution target, has the effect of reducing attempted collusion. This makes sense, since the higher bids resulting from collusion mean that fewer bids are likely to be accepted, making it more difficult to reach the contribution target. If participants value receiving this bonus payment, they have a lower incentive to attempt to collude. Finally, there is weak evidence from these results that higher social capital may reduce attempts to

collude (the p-value for the coefficient on HIGHSC is 0.11). This result, which is nearly significant at a 10% confidence level, parallels the puzzling finding that observed leadership behaviour in general is less likely in high social capital groups.

Analysis of Bid Functions

The analysis of the effect of leadership on bid functions begins with the same model estimated in the previous chapter on social capital.[17] The results of this fixed effects estimation are shown in Table 12 (this is the same as Table 3 in Chapter 3). In this case, we are particularly interested in the variables LAGLEAD and GLEAD. LAGLEAD is a dummy variable indicating that leadership behaviour was observed during the *previous* experimental session (the lag of LEADER in Table 10). This variable is lagged because during each period, participants generally submitted their bids quite quickly and then chatted while waiting for and viewing the auction results. Thus, the effect of this communication is likely to affect bids in the subsequent period. GLEAD is the interaction term between LAGLEAD and a dummy indicating the existing student group treatment. It is included to see if the effect of leadership is stronger in an existing social group.

General observations regarding UNITCOST, LNPER and the social capital variables need not be repeated, since they were discussed in Chapter 4. In terms of leadership, this regression shows that while observed leadership behaviour has no statistically significant effect on bids by itself (LAGLEAD), it has a strongly significant and large impact when it takes place in an existing social group (GLEAD). When a leader in an existing group attempts to influence the behaviour of others, participants respond by drastically increasing their bids. It appears that, at least in this context, an existing social network is necessary for leaders to have an impact on the behaviour of others. Unfortunately, it also appears that leaders have a negative impact on auctions by inducing other

[17] As noted in Chapter 3, this analysis and all following analysis excludes periods 0, 1, 6, and 11. This regression also excludes the session in which the group perfectly colluded throughout.

participants to raise their bids. This is likely to negatively impact the auction outcomes, an effect which will be further explored later in this chapter.

Table 12: Effect of Social Capital & Leadership on Bid Functions

Dependent variable: Individual bid amount

Variable	Coefficient (Standard Error)
UNITCOST	0.802*** (0.0765)
LAGLEAD	3.34 (3.10)
GLEAD	19.355** (8.08)
SCADOPT	0.00847 (0.00941)
GADOPT	-6.28*** (2.25)
LNPER	2.13 (1.50)
CONSTANT	-0.519 (5.69)
sigma_u[a]	23.4
sigma_e[b]	48.6
rho[c]	0.187
R^2 within	0.0464

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

[a] Panel-level standard deviation

[b] Standard deviation of ε_{it}

[c] Fraction of variance due to individual effect u_i

What specific types of leadership behaviour are responsible for this increase in bids? To answer this question, the next regression splits observed leadership into the four kinds of leadership behaviour that were summarized in Table 10: collusion, explanation, food bank reminders, and influence. Each variable is lagged by one period, giving the variables LAGCOLL, LAGEXPL, LAGFOOD, and LAGINFL. Each of these variables is also presented as an interaction with GROUP, to see how the effects of these specific leadership behaviours differ when taking place in an existing group; these variables are GCOLL, GEXPL, GFOOD, and GINFL. Other variables are the same as in the previous regression. The results are presented in Table 13.

Table 13: Effect of Specific Types of Leadership on Bid Functions

Dependent variable: Individual bid amounts

Variable	Coefficient (Standard Error)
UNITCOST	0.798*** (0.0762)
LAGCOLL	-0.368 (6.54)
LAGEXPL	8.76** (4.04)
LAGFOOD	-3.13 (5.59)
LAGINFL	-1.77 (4.46)
GCOLL	36.7*** (10.7)
GEXPL	-8.37 (6.76)
GFOOD	4.28 (9.01)
GINFL	13.3 (9.60)
SCADOPT	0.00927 (0.00939)
GADOPT	-4.12* (2.26)
LNPER	1.88 (1.52)
CONSTANT	-1.56 (5.68)
sigma_u[a]	21.7
sigma_e[b]	48.4
rho[c]	0.167
R^2 within	0.0568

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

[a] Panel-level standard deviation

[b] Standard deviation of ε_{it}

[c] Fraction of variance due to individual effect u_i

From these results, it appears that leadership operates in different ways depending on whether it takes place in an existing social group or not. One impact appears to come from providing explanations. The coefficient on LAGEXPL is positive and significant, indicating that leaders who explain the rules of the game to other participants cause them to raise their bids, whether or not there is an existing social group. Presumably, participants who understand the auction better are more confident in submitting higher bids. There is no statistically significant

difference when such explanations take place in an existing group. The second impact appears to happen only in existing groups, and results from collusion. The coefficient on GCOLL is statistically significant and large in magnitude. This indicates that leaders of existing social groups are able to successfully convince their followers to collude by raising their bids. Without these existing social connections, however, attempts at collusion are less likely to be successful.

These findings appear to point to the importance of trust in the functioning of both leadership and social capital. One would expect that in an existing group, there would be a higher level of trust between participants and leaders than in a group of individuals who do not know each other. Successful collusion requires trust between individuals, since even one defection from the agreement can cause a complete breakdown of collusion. Thus, in this experiment, leaders are seen to successfully encourage collusion in existing groups, but have no significant impact in terms of collusion in groups that did not know each other. Explanation of the rules of the game, on the other hand, requires a lower level of trust, especially since the other participants can also hear what the leader says and are quite likely to step in if the leader says something misleading. Thus, leaders are able to have an impact on bids through explanation, even in groups that do not know each other.

Analysis of Auction Outcomes

As for the bid functions, we begin analysis of the impact of leaders on auction outcomes by reproducing, in Table 14, the fixed effects estimation from Table 5 in Chapter 3. [18] Recall that the three dependent variables are PMAR (percentage of maximum abatement realized), POCER (percentage of optimal cost effectiveness realized) and PRENT (percentage of total payment captured as rent. Again, the variables of interest are LAGLEAD and GLEAD, as defined above.

[18] As noted in Chapter 4, this analysis excludes periods 0, 1, 6, and 11. It includes all experimental sessions.

Table 14: Effect of Social Capital & Leadership on Auction Outcomes

Variable	Coefficient (Standard Error)		
	PMAR	POCER	PRENT
LAGLEAD	-9.23* (5.06)	-19.1* (10.0)	11.3* (64.8)
GLEAD	-3.61 (12.5)	31.6 (24.8)	-4.44 (16.0)
SCADOPT	-0.0159 (0.0151)	-0.0227 (0.0299)	0.0168 (0.0194)
GADOPT	6.63** (3.01)	11.8** (6.00)	-6.00 (38.9)
LNPER	-6.11*** (2.35)	-12.9*** (4.66)	8.66*** (3.02)
CONSTANT	116*** (8.92)	232*** (17.7)	-53.6*** (11.5)
sigma_u[a]	19.3	0.86.2	26.8
sigma_e[b]	26.0	0.51.4	33.3
rho[c]	0.355	0.738	0.394
R^2 within	0.0532	0.0518	0.0461

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

[a] Panel-level standard deviation

[b] Standard deviation of ε_{it}

[c] Fraction of variance due to individual effect u_i

These results show that leadership appears to worsen auction outcomes, as the coefficients on LAGLEAD are statistically significant at a 10% confidence level for all three outcomes. That is, observed leadership in a previous period reduces the environmental benefit achieved by, and the cost effectiveness of, an auction. It also increases the amount of information rent that landowners are able to capture. The magnitudes of the impacts on each impact are quite similar; environmental benefit is reduced by 9% in an auction following observed leadership compared to an auction where no leadership was observed in the previous period; cost effectiveness is reduced by 19%, and rent is increased by 11%.

These findings are roughly in agreement with the preceding analysis of bid functions. That analysis found that leaders tend to cause participants to raise their bids, by explaining the auction procedures and by encouraging collusion in existing groups. While there appears to be no significant difference between

auctions run with existing groups and auctions where participants did not know each other in this case, this lack of precision may simply be a result of having fewer observations in this dataset.

As in the bid analysis, the various types of leadership can be separated to investigate the specific ways in which leaders might affect bidder behaviour. Table 15 presents the results of fixed effects regressions for each auction outcome, using the specific leadership variables defined in the bid functions analysis above. Unfortunately, it appears that the data do not provide any strong evidence regarding specific leadership mechanisms, probably because of the large number of variables required for this model. While the conclusions for GADOPT hold, albeit more weakly, the only leadership variable that is statistically significant is GEXPL (for POCER and PRENT). Furthermore, the implications of GEXPL are opposite of those found in all previous analysis; here it appears that the provision of explanations in an existing group improves cost-effectiveness, while reducing the amount of rent that participants are able to capture. Given the overall weaker results from this model, it is probably unsafe to draw many conclusions from it.

Table 15: Effect of Specific Leadership Types on Auction Outcomes

Variable	Coefficient (Standard Error)		
	PMAR	POCER	PRENT
LAGCOLL	-0.249 (10.8)	-26.1 (20.9)	16.8 (13.7)
LAGEXPL	-5.47 (6.58)	0.756 (12.8)	1.55 (8.38)
LAGFOOD	5.86 (9.38)	-25.0 (18.2)	11.3 (11.9)
LAGINFL	-7.72 (73.5)	7.24 (14.2)	-5.47 (9.35)
GCOLL	-7.13 (15.9)	34.4 (31.3)	-21.6 (20.5)
GEXPL	6.16 (10.4)	54.4*** (20.2)	-26.5** (13.2)
GFOOD	12.4 (13.0)	31.6 (25.6)	-16.2 (16.8)
GINFL	12.9 (15.1)	-4.28 (29.5)	7.23 (19.3)
SCADOPT	-0.0168 (0.0152)	-0.0234 (0.0295)	0.0178 (0.0194)
GADOPT	5.36* (3.17)	10.4* (6.29)	-5.27 (4.13)
LNPER	-6.21*** (2.40)	-11.6** (4.67)	7.94*** (3.06)
CONSTANT	114*** (8.99)	227*** (17.5)	-49.3*** (11.5)
sigma_u[a]	21.6	87.4	29.0
sigma_e[b]	26.1	50.6	33.2
rho[c]	0.406	0.749	0.432
R^2 within	0.0639	0.0990	0.0687

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

[a] Panel-level standard deviation

[b] Standard deviation of ε_{it}

[c] Fraction of variance due to individual effect u_i

Effect of Group Payments Treatment

Finally, some interesting results are obtained by looking at how leadership interacts with the group payments treatment, which provided bonus payments to participants if they reached a target level of social contributions. As before, the estimations are reproduced from Chapter 3, paying attention this time to the leadership variables.

Table 16 presents the results of a fixed effects regression for individual bids, with each social capital and leadership variable also interacted with a

"Group Payments" dummy variable (GP). In particular, GPLEAD is the interaction of LAGLEAD and GP, and GPGLEAD is the interaction of GLEAD and GP.

Table 16: Effect of Group Payments on Bid Functions

Dependent variable: Individual bid amounts

Variable	Coefficient (Standard Error)
UNITCOST	0.802*** (0.764)
LAGLEAD	8.89** (4.39)
GPLEAD	-11.0* (6.27)
GLEAD	-12.6 (12.4)
GPGLEAD	53.2*** (16.7)
SCADOPT	0.0121 (0.0131)
GPADOPT	-0.465 (1.49)
GADOPT	1.37 (4.11)
GPGADOPT	-9.54* (4.94)
LNPER	1.62 (1.52)
CONSTANT	-1.65 (5.71)
sigma_u[a]	22.7
sigma_e[b]	48.6
rho[c]	0.180
R^2 within	0.0510

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

[a] Panel-level standard deviation

[b] Standard deviation of ε_{it}

[c] Fraction of variance due to individual effect u_i

First, note that the average impact of leadership in a previous period (LAGLEAD) is positive and significant, indicating that participants tend to raise their bids as a result of communication intended to influence them in some way. However, adding a group payment mechanism (GPLEAD) offsets this effect, with bids decreasing by a slightly greater magnitude than the original effect. This finding makes sense, since higher bids mean less bids can be accepted within the

auction budget and therefore make it less likely that the group target will be achieved. Leaders who want to reach this target are likely to encourage participants to lower, not raise, their bids. The fact that these coefficients offset each other probably explains why LAGLEAD is not significant when the group payments mechanism is not taken into account.

However, when leadership occurs in the context of an existing group facing a group payment mechanism (GPGLEAD), the result is to drastically increase bids. This may imply that the leaders in these groups expected a greater overall benefit to be achievable by collusion rather than the group bonus payment, and therefore encouraged individuals to raise their bids. The coefficient on GLEAD is not significant in this case, implying that the previous findings regarding GLEAD are mainly driven by the effects of existing groups within the group payments treatment.

Tables 17 and 18 reproduce Tables 7 and 8 from Chapter 3. Recall that Table 17 isolates the behavioural impacts of the group payments mechanism, while Table 18 includes the effects of the bonus payments themselves on cost-effectiveness and other auction outcomes.

Table 17: Effect of Group Payments on Auction Outcomes, Excluding Bonus Payments

Variable	Coefficient (Standard Error)		
	PMAR	POCER	PRENT
LAGLEAD	-5.28 (7.28)	-39.7*** (14.5)	21.6** (9.51)
GPLEAD	-6.82 (10.2)	41.1** (20.3)	-15.8 (13.3)
GLEAD	4.50 (17.4)	106*** (34.7)	-51.9** (22.8)
GPGLEAD	-10.1 (25.6)	-134*** (50.9)	88.2*** (33.5)
SCADOPT	0.00492 (0.0214)	-0.0181 (0.0425)	0.0800 (0.0279)
GPADOPT	-3.15 (2.38)	-1.36 (4.73)	2.22 (3.11)
GADOPT	3.10 (4.14)	9.76 (8.32)	-3.42 (5.47)
GPGADOPT	2.06 (6.04)	-13.7 (12.1)	4.89 (7.94)
LNPER	-6.63*** (2.38)	-12.6*** (4.74)	8.70*** (3.12)
CONSTANT	117*** (8.93)	250*** (17.8)	-62.9*** (11.7)
sigma_u[a]	18.3	74.5	34.7
sigma_e[b]	26.0	51.6	34.0
rho[c]	0.332	0.675	0.511
R^2 within	0.0516	0.0753	0.0690

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

[a] Panel-level standard deviation

[b] Standard deviation of ε_{it}

[c] Fraction of variance due to individual effect u_i

Table 18: Effect of Group Payments on Overall Auction Outcomes

Variable	Coefficient (Standard Error)		
	PMAR	POCER	PRENT
LAGLEAD	-5.29 (7.28)	-39.9*** (14.2)	21.8** (9.22)
GPLEAD	-6.82 (10.2)	40.9** (19.9)	-20.9 (13.0)
GLEAD	-4.64 (16.9)	73.3** (32.9)	-33.5 (21.4)
GPGLEAD	-0.918 (25.3)	-96.7** (49.2)	67.7** (32.0)
SCADOPT	0.00464 (0.0214)	-0.0186 (0.0417)	0.00832 (0.0271)
GPADOPT	-3.13 (23.8)	-1.06 (4.64)	1.55 (30.1)
GADOPT	7.01* (40.9)	24.2*** (8.05)	-12.0** (5.24)
GPGADOPT	-1.84 (6.01)	-27.7** (11.8)	14.0* (7.65)
LNPER	-6.62*** (2.38)	-11.9** (4.64)	8.25*** (3.02)
CONSTANT	116*** (8.94)	232*** (17.4)	-5.33*** (11.3)
sigma_u[a]	20.9	87.1	39.0
sigma_e[b]	26.0	50.6	32.9
rho[c]	0.392	0.747	0.583
R^2 within	0.0632	0.0914	0.0728

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

[a] Panel-level standard deviation

[b] Standard deviation of ε_{it}

[c] Fraction of variance due to individual effect u_i

Looking first at Table 17, it seems that the behavioural impacts on the auction outcomes are generally in agreement with the findings from the bid function analysis, although there is a loss in precision of the estimates. Since none of the treatment variables are significant for PMAR, analysis will focus on the POCER and PRENT models. By itself, leadership (LAGLEAD) has a negative impact on both these outcomes, reducing the cost-effectiveness and increasing the rent captured by landowners. However, leadership under the group payments mechanism (GPLEAD) offsets this effect, at least for POCER. These effects mirror the observed effects in the bid function, where leadership by itself raised bids but leadership with group payments lowered them. Leadership within an

existing group (GLEAD) significantly improves both outcomes; this finding is in agreement with the negative sign of the coefficient in the bid function analysis, although this variable was not statistically significant in the bid function. Finally, auction outcomes are significantly worse when leadership occurs in an existing group with a group payments mechanism (GPGLEAD); again, this agrees with the positive sign on this coefficient in the bid function. Thus, the behavioural impacts of leadership show up in the same way in both the bid function and the analysis of outcomes.

Looking now at Table 18, it appears that the effects of the group payments mechanism also show up quite similarly in the model presenting overall outcomes. The signs, magnitudes, and p-values of the coefficients for the leadership variables are quite similar in the two models, indicating that the structural impacts of the group payments mechanism either work in the same direction as the behavioural effects, or have little impact.

Conclusion

Leadership has been shown to have a significant impact on bidder behaviour and conservation auction outcomes, in these experiments. However, the impact of leadership is also found to interact in interesting ways with social capital. In particular, within existing groups, it appears that leaders can successfully convince followers to collude, raising their individual bids. Outside of existing groups, however, such collusion does not occur. It appears that a certain level of trust, an element of collective social capital, is necessary in order for a leader to promote collusion.

However, even in groups without existing social networks, leaders can have an impact by clarifying the rules of the game. When leaders do so, other participants have a tendency to raise their bids. Thus, in combination with the collusion in existing groups, leaders have an overall negative impact on auction outcomes, reducing cost-effectiveness and environmental outcomes, and increasing the rent captured by bidders.

The addition of the group payments treatment to the analysis adds complexity to these findings. In groups of participants that do not know each other, leaders seem to encourage followers to lower their bids when they have the incentive of a group bonus for achieving the contribution target. This is the only context in which leaders have a positive impact (from the government's perspective) on behaviour within the auction, and the associated auction outcomes. However, when social networks exist, bids increase even when group payments are offered, probably indicating successful collusion; the impacts on auction outcomes are likewise negative.

Thus, these findings contribute to the sparse economic literature on the impacts of leadership, showing that leaders can have a significant impact on the behaviour of followers in the specific context of a conservation auction, and highlighting different types of leadership which can prove effective in this setting. The experiment has also shown that, given the opportunity, individuals can and will behave as leaders even if they face the same information and choices as other participants. This opens up a new way to study the impact of leaders in an experimental setting, by varying the composition of groups taking part in the experiment. Given the apparent weakness of the MLQ in predicting individuals likely to behave as leaders in this context, however, further research into alternative ways of identifying leaders may prove useful.

Chapter 5: Do Conservation Auctions Cause Crowding Out?

So far, this study has shown that the social context in which a conservation auction takes place, specifically the level of social capital and leadership among auction participants, makes a difference to both bidder behaviour and auction outcomes. In this chapter, we shift focus slightly to examine how the existing pro-social or pro-environmental motivations of landowners may affect their response to the implementation of a conservation auction.

Crowding Out and Conservation Auctions

Conservation auctions represent an "external" motivation for landowners to adopt BMPs. They are useful in situations where landowners provide less than the socially optimal amount of environmental improvement; it is hoped that the financial incentive provided by the auction will increase the willingness of landowners to contribute to societal goals.

However, in any given situation, it is possible that some landowners will already have adopted BMPs, based on their own "internal" pro-social or pro-environmental motivations. Individuals may voluntarily undertake pro-social activities, including improving environmental quality, for a number of reasons, including altruism, a concern for social norms, or a concern for fairness (Nyborg and Rege 2003a). A recent survey of Australian farmers found that environmental stewardship was rated higher as a motivation for conservation adoption decisions than economic considerations (Greiner and Gregg 2011). A variety of other studies have found that farmers highly value conservation and stewardship (Ahnstrom et al. 2009). Thus, it is possible that some farmers may adopt conservation activities without external monetary incentives being provided.

Unfortunately, a growing body of evidence shows that pro-social motivations appear to be crowded out by the introduction of external mechanisms,

including market-based instruments that provide financial incentives to change behaviour (Goeschl and Perino 2009, Reeson and Tisdell 2010). In other words, external incentives may actually reduce the amount of social or environmental benefits that individuals are willing to provide voluntarily. This paper contributes to the evidence on this issue by examining whether this is also the case for conservation auctions, which represent a specific type of market-based instrument.

Crowding out has been studied in the case of competitive tenders, using a public goods game in an experimental setting (Reeson and Tisdell 2010). While this study follows Reeson and Tisdell's general format, it provides a closer parallel to a real conservation auction by having BMP adoption result in real donations to an environmental charity, rather than redistributing contributions among participants in the experiment. This reflects the fact that in conservation auctions, the benefits of BMP adoption are primarily experienced by wider society, not the farmers who participate directly in the auction.

We find that conservation auctions, like some other external incentives, do appear to cause crowding out of voluntary environmentally friendly behaviour. After an auction is introduced and then removed, BMP adoption by participants who experienced the auction fall in relation to adoption prior to the auction. In contrast, in a control group that does not experience the auction and is only given the option of voluntary adoption, average adoption rises slightly over the course of the experiment. Thus, it appears that the designers of conservation auctions will need to take the possibility of crowding out into account and seek to minimize the potential negative impacts on voluntary environmentally friendly behaviour.

Theoretical Insights into Crowding Out

The phenomenon of crowding out has been extensively studied in the social psychology literature, and has more recently gained attention among economists. Bruno Frey (1997) has played a role in bridging the two fields through his "motivation crowding theory." This theory follows social psychology in drawing a distinction between internal motivations and external (often

monetary or regulatory) motivations. Introducing an external motivation is thought to affect the internal motivations for performing a task, and thereby to influence individual behaviour (Frey 1993, 1997). For example, providing a monetary payment for performance of a task can crowd out internal motivation to perform the task. By shifting the supply curve for the behaviour to the left, a monetary payment can reduce the amount supplied even though the price is higher (Frey and Jegen 2001).

Economists have developed a range of theories to account for voluntary pro-social behaviour, putting some flesh to Frey's concept of internal motivations (Nyborg and Rege 2003a). Each of these theories provides insight into why voluntary pro-social behaviour may be crowded out by introducing external motivations through a market-based mechanism. Before describing these theories, a simple conceptual model may help to focus the discussion.

Levitt and List's (2007) model that incorporates moral concerns into utility can be adapted for this purpose (for a much more comprehensive theoretical model, see Benabou and Tirole (2006)). Following Levitt and List (2007), we will assume that the utility associated with a particular action includes both a wealth component and a moral component, which are additively separable. The moral component *M* includes the action *a*, the magnitude of its impact *v*, and the social motivations for undertaking it *s*. The wealth component *W* includes the action and its impact. The moral component carries a weight of *b* and the wealth component is weighted *c*. This gives the following utility function:

$$U_i = bM_i(a, v, s) + cW_i(a, v)$$

The moral component of utility can be interpreted as Frey's "internal motivation." The following discussion suggests a variety of interpretations for how this portion of utility may be diminished by the introduction of external incentives.

The theories of social norms and social approval (Hollander 1990, Rege 2004, Nyborg et al. 2006, Levitt and List 2007, Nostbakken 2009) argue that individuals gain utility from undertaking actions of which society approves. For example, individuals contribute to public goods because the social approval

resulting from obeying a social norm in favour of contributing provides them with positive utility. Thus, landowners may adopt BMPs because the gain in the moral component of utility, resulting from obeying a social norm in favour of these practices, outweighs the utility loss from the wealth component.

However, it is possible that when landowners receive payment for adopting these practices, such as through a conservation auction, others will assume that landowners are motivated not by social norms, but by external financial rewards. This may be interpreted as reducing *s* in the model above. Benabou and Tirole's (2006) theoretical model of crowding out explicitly addresses this issue and shows that the supply curve for pro-social behaviour can actually be downward-sloping in some circumstances. In this model, rewards introduce "noise" into the observation of pro-social attitudes. This makes it more difficult for individuals to achieve a good reputation by undertaking pro-social behaviour, and thus reduces the motivation for such behaviour.

Theories of fairness or reciprocity (Rabin 1993, Levine 1998, Falk and Fischbacher 2006, Segal and Sobel 2007) hold that individuals contribute to public goods because they gain utility from reciprocating the "kind" actions of others who have also contributed. Thus, landowners may adopt BMPs because others are doing so, and the utility of reciprocating (by contributing to environmental quality that is enjoyed by those others) outweighs the loss in profit. In the model above, this implies that the moral weight *b* would have a positive sign if others show kindness by adopting BMPs, and a negative sign if they do not.

When landowners gain individual financial rewards from their adoption of BMPs, however, this may detract from the perception that the actions are being undertaken out of a sense of other-regarding kindness, and thus reduces the incentive to reciprocate. In the model above, it reduces *b*. In fact, landowners who have already adopted the BMPs are excluded from participation in the program, this may be perceived as unfairness or unkindness on the part of the program administrators. This may give *b* a negative sign, inducing these landowners to negatively reciprocate by stopping their contribution to the environmental good.

Fairness theories also include theories of inequality aversion (Bolton 1991, Fehr and Schmidt 1999, Bolton and Ockenfels 2000), which argue that individuals receive disutility from being much worse off or better off than others. Thus, if others contribute to a public good, an individual may contribute as well because this decreases inequality. Landowners may therefore adopt BMPs because others are doing so, and this reduces the inequality resulting from lost profits. Introducing external financial payments, however, may eliminate this source of inequality and thus the motivation to contribute. Furthermore, because excluding landowners who have already contributed may make them significantly worse off in relation to those who are included in the program, they may have an incentive to stop implementing the BMPs in an attempt to reduce this inequality.

A third set of theories explains contributions to public goods in terms of altruism (Schwartz 1970, Becker 1974, Schmid and Robison 1995). In this view, the well-being of others contributes to an individual's utility. If contributions to a public good increase the well-being of others, an individual has an incentive to contribute. Thus, farmers may adopt BMPs in order to improve the well-being of others in the community who benefit from the improvement in environmental quality. A variant, impure altruism (Andreoni 1990), states that individuals receive utility ("warm glow") simply from the act of contributing to a public good. In this case, farmers would adopt BMPs because they receive utility directly from doing so which, again, outweighs the loss in profit. In either case, the variable *s* in the model above can be interpreted to represent altruism.

It is possible that inducing other farmers to adopt BMPs through a conservation auction will crowd out these motivations; that is, the moral component of utility (M) may be satisfied by the actions of others. It can be shown (Warr 1982) that in the case of pure altruism, private contributions to a public good are (theoretically) completely crowded out by government contributions. Thus, landowners may stop implementing BMPs when others, paid through the auction, adopt them instead. In the case of impure altruism, crowding out does occur, but incompletely (Andreoni 1990).

Finally, theories of commitment (Sen 1977, Sugden 1984, Nyborg 2000) argue that individuals contribute to a public good even when it does not increase their utility, because they are willing to place constraints on their own private utility maximization for the sake of the common good. In this view, farmers will adopt BMPs simply because they believe it is the right thing to do, from a societal perspective, even if it decreases their individual utility. In this case, it is not clear whether introducing a conservation auction will cause crowding out to occur, since this will depend on the form of the constraint being adopted by the landowner. If the landowner simply believes that adopting the BMP is the right thing to do, crowding out is unlikely; however, if the landowner believes that environmental quality should meet a certain threshold, it is possible that achieving this threshold through the auction will remove the landowner's self-imposed responsibility to use the BMPs. Tenbrunsel and Messick (1999) offer an interesting variation on this argument, suggesting that the introduction of sanctions for environmental damage changes the frame of reference from an ethical to a business decision. In our model, this equates to reducing the weight on the moral component of utility to zero. A conservation auction, which introduces payments for providing environmental benefits, could have the same effect.

Empirical Evidence on Crowding Out

A broad range of studies have found evidence that individual motivations for action may be crowded out by the introduction of external incentives. These include both social psychology studies (Deci et al. 1999) and economic studies (Frey and Jegen 2001). A number of these studies are particularly relevant to the current research since they deal with environmental contexts and/or market-based instruments.

Environmental problems are often addressed through command-and-control regulations. Cardenas, Stranlund and Willis (2000) study the effect of regulation on crowding out through a laboratory experiment in rural Colombian villages. The experiment is set up as a public bads game where time spent collecting firewood in a shared forest damages local water quality. The

introduction of an imperfectly enforced regulation, limiting the time that can be spent collecting firewood, causes participants to move towards more self-interested choices and away from the other-regarding choices that are socially efficient. In a similar experiment carried out in Namibia, where a common pool resource game is framed in terms of livestock grazing on communal lands, Vollan (2008) draws the same conclusion. Tenbrunsel and Messick's (1999) study with business students also finds that weak sanctions reduce cooperative efforts to prevent pollution. Reeson and Tisdell (2008) study the impact of regulation, using a public goods game. Partway through the game, a regulation is introduced that specifies a minimum contribution to the public good. When the regulation is dropped later in the game, contributions to the public good become significantly lower than contributions before the regulation was introduced, and significantly lower than contributions in a comparison treatment without regulation. Thus, command-and-control approaches appear to cause crowding out effects.

Market-based instruments, on the other hand, use monetary incentives to induce pro-social behaviour. Gneezy and Rustichini (2000) study this in a field experiment involving daycares. They find that when a monetary fine is imposed on parents who are late picking up their children, the number of late pickups doubles. Furthermore, when the fine is removed, the number of late pickups remains higher than it was before the fine was introduced. This result appears to indicate that the monetary incentive represented by the fine has crowded out social norms for picking children up on time. In another study of monetary incentives, directly related to the environment, Frey and Oberholzer-Gee (1997) survey two samples of Swiss citizens about their willingness to accept a nuclear waste repository in their community. When no compensation is offered, 50.8% of respondents in one sample and 41% of respondents in another state that they would accept the repository. When compensation is offered, the acceptance rate falls to 24.6% and 27.4%, respectively. They suggest that the reason for the decline is a loss in "public spirit" due to the offered compensation.

Goeschl and Perino (2009) compare crowding out effects for both these types of external motivations. In a laboratory study where participants have the

ability to give up monetary rewards in return for real contributions to carbon offsets, they find that command-and-control mechanisms do not exhibit crowding out effects, but that taxes do.

Finally, one study has examined the effects of crowding out in the context of a competitive tender (auction). In a laboratory public goods game, Reeson and Tisdell (2010) introduce a tender which allows participants to submit bids for financial payments in return for contributing to the public good. They find that contributions from unsuccessful bidders fall significantly during the tender periods, and do not recover after the tender is removed. In contrast, contributions in a control treatment with no tender do not vary significantly over the course of the experiment. Thus, there is a growing body of evidence showing that market-based instruments, along with other external motivations, can cause crowding out of pro-social behaviour.

The current study adds to this body of evidence, with particular attention to conservation auctions. It follows the general structure of Reeson and Tisdell's (2010) study. However, it is designed specifically to simulate a conservation auction, rather than a general public goods game. First, our experiment is framed explicitly as a conservation auction. Participants are asked to think of themselves as landowners and are asked to make a decision regarding the adoption of BMPs on their land. Second, the BMP costs and benefits used in the experiment are drawn from real data on the costs of BMP adoption by Canadian farmers. Third, BMP adoption in the experiment results in real donations to an environmental charity, instead of contributions that are redistributed among participants in the experiment. This provides a closer parallel to real conservation auctions, where many of the environmental benefits of BMP adoption accrue to wider society, not primarily to the farmers who take part in the auction. Conservation auctions are appropriately used for BMPs where the public net benefits are greater than the private net benefits (Pannell 2008). For example, BMPs aimed at water quality improvement benefit downstream water users, while BMPs for habitat protection benefit individuals who value the species thus protected. The following section explains the experimental design in more detail.

Experimental Design

To examine the effect of conservation auctions on voluntary pro-social behaviour, two experimental treatments were designed. Both treatments create a situation where individuals have the opportunity to make voluntary contributions, that are costly to them but that provide benefits to society. Multiple rounds of the experiment were conducted with each group of participants. The control treatment asked participants to make voluntary, uncompensated contributions throughout. The auction treatment began with the same mechanism as the control, but then introduced an auction mechanism to compensate contributors during the middle rounds of the experiment, and removed it for the final rounds. Analysis focuses on differences in voluntary contributions between the two treatments, both during and after the auction.

Both treatments were framed in terms of environmentally friendly conservation decisions made by landowners. Participants were told that they each represented a landowner with a hypothetical farm to manage (for full instructions, see Appendix C). They were asked to make decisions regarding the adoption of (unspecified) BMPs on their farm, and were told that the adoption of BMPs results in environmental benefits for society. To represent this in the experiment, participants were told that if they chose to adopt BMPs, the experimenters would make a real donation to an environmental charity on their behalf.

Each round, participants were endowed with $2 (their "farm profits"). Each participant was told their individual costs for various levels of BMP adoption (0%, 25%, 50%, 75% and 100% of "the practices that will work on your farm"). These costs were drawn from real data on BMP adoption by Canadian farmers (Boxall et al. 2009), scaled to match the $2 endowment, and ranged from $0.04 to $0.76 per 25% increment. The costs were different for each participant and the set of costs was redistributed among participants each round. Participants were also told the social benefit associated with these levels of adoption, which was fixed at $0.50 per 25% increment. Thus, each 25% adoption level resulted in a real donation of $0.50 to a Canadian environmental charity. Based on these costs and benefits, participants were invited to choose their desired level of BMP

adoption. The cost of the chosen adoption level was deducted from the $2 endowment, and participants were told their earnings for that round and the monetary donation that would be made on their behalf. Participants were not allowed to adopt BMPs that they were unable to pay for out of their endowment.

The control treatment used 12 rounds, each following the procedure above. This simulated voluntary conservation activity, with no compensation provided. In the auction treatment, the first four founds also followed the procedure above. Between the fourth and fifth rounds, the auction mechanism was introduced (participants in both the control and treatment groups had been told that the procedures might change during the experiment, but not how or when). Participants were told that the government was now willing to provide compensation for the adoption of BMPs. They were invited to submit an offer specifying the adoption level they would commit to, and their desired compensation. They were informed that the offers would be ranked according to the per-unit compensation desired, and that the best offers would be accepted up to an (unrevealed) budget cap.

The auction was implemented during the middle four rounds of the auction treatment. Participants submitted their bids and were then told whether or not their bid was successful. Successful bidders received their desired compensation, in addition to their $2 endowment. Unsuccessful bidders, or participants who chose not to submit a bid, received only their $2 endowment. Each participant was then asked to choose their adoption level for that round. Successful bidders were required to choose at least the adoption level specified in their bid, but were also allowed to choose a higher level; however, additional adoption did not result in additional compensation. Unsuccessful bidders could choose any adoption level, as long as they could pay for it using their endowment. Thus, voluntary adoption behaviour was still an option for any participant.

After round eight, participants were informed that the government would no longer provide compensation for BMP adoption, and the procedure then went back to the original design. This continued for the final four rounds of the auction treatment.

The experiment was programmed using Z-Tree (Fischbacher 2007), software designed for conducting economic experiments. Each participant was seated at an individual computer terminal and made their decisions anonymously, using the computer software. Their screen displayed information about their costs and benefits, decisions in past rounds, and an ongoing tally of earnings. Total earnings were paid in cash at the end of the experiment and participants were also informed about the total donation to be made on their behalf.

Because social factors such as altruism, social norms and reciprocity are thought to play a role in voluntary pro-social behaviour, the experiment included several elements designed to simulate a real social situation. First, participants were told the adoption level chosen by each other participant in the past round. To protect confidentiality, this information was displayed by participant number, which was unknown to the other participants. This information allows for the formation of social norms, which in economic theory are generally represented as arising from the "average" behaviour of members of a group. It also allows for reciprocity or fairness to play a role, which can only happen if participants know the behaviour of others.

Second, participants were allowed to communicate throughout the experiment, both through an electronic chat function in the experimental software and by simply talking out loud. This allows for expressions of social approval or disapproval, which are an important part of the process through which social norms are thought to affect behaviour. The electronic chat allowed participants to relate communications directly to actions of other participants, since individuals were identified in the chat by their participant number. Allowing verbal chatting provided participants with a more natural way to communicate, especially for those who might not have been comfortable using the electronic chat.

Third, the repeated experimental rounds allowed individuals to use and be influenced by information from the previous rounds regarding the decisions made by others. This is necessary for the formation of social norms, expression of social approval/disapproval, and reciprocity, because of the fact that all

participants made their adoption decision at the same time in any given auction round.

Four sessions for each of the two experimental treatments were held. There were 10 participants in each experimental session, making a total of 80 participants in the experiment. Participants were drawn from a database of volunteer experimental participants at the University of Alberta. Most participants were students, although a few were staff or faculty connected with the university. The sample is approximately gender balanced, with 38 females and 42 males.

Results and Analysis

Based on the experimental design described above, the crowding out hypothesis implies that voluntary (uncompensated) BMP adoption will be lower after an auction is introduced and then removed, than in the corresponding rounds for the control treatment with no auction. The reason that behaviour in the auction treatment is compared to behaviour in the control treatment, instead of simply comparing behaviour before and after the auction, is that there may be other factors (such as boredom, fatigue, satiation, or wealth effects) that induce changes in behaviour over time. Because these factors should be the same for the control and experimental treatments, comparison of the two treatments allows us to take these factors into account.

This means that the analysis of behaviour will rely on a difference-in-differences approach (Card and Krueger 1994). This approach accounts for the effects of time trends which are common to both groups and do not depend on the experimental treatment. It also accounts for any differences between the groups unrelated to the treatment, although we do not expect there to be any selection bias or consistent unobserved differences between the two groups. Although participants did self-select into the experimental time-slots, these slots were varied to ensure there was no consistent pattern in the timing of experimental treatments, and participants were not told which treatment they were signing up for.

The behaviour that we observe, level of BMP adoption, is censored at both ends. There are five possible levels, corresponding to 0%, 25%, 50%, 75%,

and 100% adoption. Thus, the lowest level of adoption possible is 0% (with an adoption cost of $0.00, resulting in a donation of $0.00) and the highest possible level is 100% (with a variable adoption cost, resulting in a donation of $2.00). These levels are coded as 0, 1, 2, 3, and 4. Thus, a Tobit model with a lower limit at 0 and an upper limit at 4 was used to estimate the difference-in-differences model.

To account for the panel structure of the data (12 observations for each participant), a random effects Tobit model was used. The random effects model is appropriate since the dependent variables are all related to the externally imposed treatment, and thus should not be correlated with the individual-specific effects.

The econometric model is:

$$ADOPTION_{it} = \beta_0 + \beta_1 UNITCOST_{it} + \beta_2 BIDPAY_{it} + \beta_3 TREAT_{it} + \beta_4 SECOND_{it} + \beta_5 THIRD_{it} + \beta_6 TREAT2_{it} + \beta_7 TREAT3_{it} + \varepsilon_i$$

UNITCOST and BIDPAY account for, respectively, the cost per adoption level (25% increments) and the total compensation received from the auction, if present. We expect the coefficient on UNITCOST to be negative and the coefficient on BIDPAY to be positive. The rest of the variables implement the difference-in-differences design. TREAT is a dummy variable equalling one if the observation comes from a session using the auction treatment. This accounts for any unobserved group differences. SECOND is a dummy representing the second four periods during the experiment (during which the auction was implemented in the auction treatment). THIRD is a dummy representing the third four periods during the experiment (after the auction was removed in the auction treatment). TREAT2 is the interaction between TREAT and SECOND, and captures any change in adoption behaviour during the periods which included an auction, that is unaccounted for by the bid payments received.

TREAT3 is the variable of main interest, as it represents the interaction between TREAT and THIRD and thus captures any change in adoption behaviour during the periods after an auction is removed, in the auction treatment. Thus, the coefficient on TREAT3 represents the crowding out effect. Our crowding out hypothesis implies that the coefficient on TREAT3 will be negative.

Two sets of results are presented in Table 19 below. The first column presents results for the complete dataset: 12 observations from each of 80 participants, making 960 observations in all. The second column presents results of a regression excluding periods 1, 5 and 9. These can be considered as "practice rounds" for each of the three main segments of the auction treatment. In experiments, it often takes a round or two before participants are familiar and comfortable with the experimental procedures, so excluding these initial rounds may result in more reliable estimates. In fact, excluding these rounds results in stronger statistical significance for the key variables, despite the fact that this model has less data (720 observations in all).

Table 19: Crowding Out of Voluntary BMP Adoption

Dependent variable: Chosen adoption level

Variable	**Coefficient (Standard Error)**	
	Full dataset	Excluding periods 1,5,9
UNITCOST	-6.756 (0.338)***	-7.251 (0.415)***
BIDPAY	2.210 (0.297)***	2.292 (0.369)***
TREAT	0.597 (0.420)	0.702 (0.444)
SECOND	0.282 (0.225)	0.209 (0.269)
THIRD	0.344 (0.226)	0.370 (0.269)
TREAT2	-0.189 (0.349)	-0.056 (0.416)
TREAT3	-0.635 (0.317)**	-0.808 (0.378)**
CONSTANT	2.788 (0.313)***	2.931 (0.336)***
sigma_u[a]	1.580 (0.156)***	1.581 (0.165)***
sigma_e[b]	1.703 (0.068)***	1.746 (0.082)***
Log likelihood	-1235.135	-926.814
Wald χ^{2} [c]	465.41	355.85

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

[a] Panel-level standard deviation

[b] Standard deviation of ε_{it}

[c] Test of null hypothesis that all coefficients are equal to zero

The results of the analysis confirm the crowding out hypothesis for this experiment. As hypothesized, the coefficient on TREAT3 is negative and statistically significant at the 5% level. This means that crowding out does in fact occur in this case; participants are less willing to adopt BMPs after an auction has been introduced and then removed, compared to the control group.

It is also interesting to note that the coefficient on TREAT2, which represents the difference in donations during the auction for the treatment group versus the control group, is not statistically significant. Once we have controlled for the fact that some participants in the auction receive compensation for their contributions (via BIDPAY), there is no significant evidence of any difference in behaviour between the treatment and control groups.

The results also indicate that participants are price-sensitive, as one would expect, with a negative coefficient on the cost per unit of adoption. The positive coefficient on BIDPAY indicates that participants are more likely to adopt BMPs when they are compensated (which is unsurprising, since they were constrained to do so).

To see the magnitude of the effect of the explanatory variables on observed behaviour, the adoption decisions made by participants, it is necessary to calculate their marginal effects. These are reported in Table 20 below, for the model excluding rounds 1, 5, and 9 (the results for the full dataset are similar). The first column reports the marginal effects conditional on ADOPTION being between 0 and 4; the second column reports the unconditional marginal effects. Marginal effects are calculated at the means for the continuous variables (UNITCOST and BIDPAY) and for the dummy variables, the difference in E(ADOPTION) when the dummy equals 0 and when it equals 1.

Table 20: Marginal Effects

Variable	Marginal effect (Standard Error)	
	Conditional	Unconditional
UNITCOST	-1.547 (0.149)***	-4.083 (0.254)***
BIDPAY	0.489 (0.089)***	1.291 (0.213)***
TREAT	0.150 (0.095)	0.394 (0.248)
SECOND	0.045 (0.058)	0.118 (0.153)
THIRD	0.079 (0.058)	0.210 (0.154)
TREAT2	-0.012 (0.089)	-0.032 (0.233)
TREAT3	-0.170 (0.079)**	-0.434 (0.193)**

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

The dependent variable is ADOPTION, which reflects five possible levels of BMP adoption (0%, 25%, 50%, 75% and 100%), coded as 0, 1, 2, 3, and 4. Focusing on the conditional marginal effects as more relevant (since the limit on BMPs applicable to a given farm is a limit that holds in reality for farmers), these results show that the magnitude of the crowding out effect is not very large, although it is certainly present and statistically significant. The average adoption level in the whole experiment, combining both treatments, is 1.47 units, corresponding to a donation of $0.74. Participants who have experienced an auction, which was then removed, provide on average 0.17 units less of adoption (corresponding to a donation of about $0.09) than participants who are at the same point in the experiment but did not experience an auction. Thus the auction appears to crowd out a small, but statistically significant, amount of voluntary pro-environmental behaviour.

The magnitude of the effects of adoption cost and auction payments show that the correspondence between the costs faced by participants and the donations they provide does not appear to be one-to-one. An extra dollar of adoption costs reduces donations by 1.547 units on average (corresponding to a donation of $0.77), which implies that even participants with fairly high costs are still sometimes willing to make donations. However, an extra dollar in auction

payments increases donations by, on average, only 0.489 units ($0.24). This may indicate that some of the participants receiving payments would have donated anyway, meaning that the auction did not increase donations by a great deal on average.

It is possible that crowding out is more likely for some groups than others. Although the panel structure of the model accounts for unobserved individual-level variables that might influence behaviour, their effect can be seen directly by including these variables explicitly in the model. Each participant in the experiment filled out a short survey providing demographic data. Because these variables are most likely correlated with the other, unobserved individual-level effects, they cannot simply be included in the random effects Tobit model. However, some insight can be obtained by splitting the sample into groups based on demographic variables. Age is excluded since there is little variation, with almost all participants being university students. Some interesting results are obtained by splitting the sample by gender, and by previous participation in an economic experiment.

Table 21 reports on model results obtained from splitting the sample into males and females. The dataset excludes periods 1, 5 and 9 as above. There were 42 male and 38 female participants. The results in Table 21 show that crowding out appears to be more prominent among males. While the coefficient on TREAT3 is negative for both males and females, it is statistically significant for males but not for females. Other results appear to be quite similar between the genders.

Table 21: Effects of Gender on Crowding Out

Variable	Coefficient (Standard Error)	
	Males	Females
UNITCOST	-8.406 (0.709)***	-6.298 (0.491)***
BIDPAY	2.343 (0.535)***	2.149 (0.508)***
TREAT	0.779 (0.575)	0.459 (0.651)
SECOND	0.361 (0.411)	0.023 (0.350)
THIRD	0.279 (0.415)	0.417 (0.348)
TREAT2	0.020 (0.648)	-0.035 (0.534)
TREAT3	-1.290 (0.619)**	-0.506 (0.468)
CONSTANT	2.704 (0.433)***	3.305 (0.508)***
sigma_u[a]	1.247 (0.202)***	1.705 (0.252)***
sigma_e[b]	1.967 (0.138)***	1.534 (0.097)***
Log likelihood	-451.27	-461.30
Wald χ^{2} [c]	172.21	180.54

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

[a] Panel-level standard deviation

[b] Standard deviation of ε_{it}

[c] Test of null hypothesis that all coefficients are equal to zero

Table 22 reports results from splitting the sample into participants who had previously participated in an economic experiment, and participants who had not. Because participants came from a database of volunteers, some had previous experience in laboratory experiments. Experiments run using this database in the past have been of various types, including some conservation auctions (it is not possible to determine precisely which type of experiments these participants had taken part in). All, however, involved individual decision-making and monetary payments based on these decisions. In this experiment, there were 26 participants who had previously participated in an experiment and 54 who indicated that they had not.

The results in Table 22 show that the crowding out effect appears to be stronger in the group that had previously participated in an economic experiment.

The coefficient on TREAT3 is significant at a 5% level for those who had participated and only at a 10% level for those who had not. The magnitude of the coefficient is also much greater for the group that had previously participated. Thus, it appears that the crowding out of intrinsic motivations may take place more readily among participants who had previously experienced a situation where they received monetary rewards for making individual decisions.

Table 22: Effects of Experience on Crowding Out

Variable	Coefficient (Standard Error)	
	Have Participated	Have Not Participated
UNITCOST	-15.034 (1.474)***	-5.506 (0.398)***
BIDPAY	5.192 (2.495)**	2.041 (0.332)***
TREAT	0.465 (1.230)	0.288 (0.450)
SECOND	0.335 (0.522)	0.266 (0.307)
THIRD	0.470 (0.496)	0.399 (0.312)
TREAT2	-0.408 (1.358)	-0.171 (0.434)
TREAT3	-1.960 (0.982)**	-0.667 (0.403)*
CONSTANT	3.878 (0.709)***	3.016 (0.375)***
sigma_u[a]	2.326 (0.437)***	1.257 (0.158)***
sigma_e[b]	1.804 (0.201)***	1.579 (0.082)***
Log likelihood	-200.39	-681.12
Wald χ^{2} [c]	109.39	244.51

*** = significant at 1% level, ** = significant at 5% level, * = significant at 10% level

[a] Panel-level standard deviation

[b] Standard deviation of ε_{it}

[c] Test of null hypothesis that all coefficients are equal to zero

Conclusion

The results of the experiment confirm that crowding out may be a problem in conservation auctions, adding to the growing body of evidence that demonstrates crowding out in other experimental and field situations. The present experiment is designed to simulate a conservation auction as closely as possible in

the laboratory, while controlling for extraneous factors that may affect behaviour and providing a counterfactual through the control treatment. The incentives provided to participants, including both individual monetary compensation and real environmental benefits to wider society (via donations to an environmental charity) mirror the incentives faced by real farmers in the field, though on a smaller scale. The experiment also provides channels through which social factors can play a role in influencing behaviour, as they would in a real field situation.

Thus, these results draw attention to an issue that should be carefully considered when designing and implementing conservation auctions. The possibility that conservation auctions may cause crowding out of voluntary pro-environmental behaviour means that they may need to be implemented with caution in some circumstances. For example, they may be more useful in communities where there is little voluntary pro-environmental behaviour in the first place, than in communities where there is more voluntary behaviour to be crowded out.

Crowding out is also most likely to be an issue in the case of temporary or short-term programs, where funding for BMP adoption is discontinued after a few years. Although initial BMP adoption costs have been covered and the landowner has gained knowledge about the new management practices, there may be ongoing maintenance or opportunity costs for some BMPs. If the auction has crowded out voluntary pro-environmental or pro-social motivations, the landowner may not be willing to bear these ongoing costs and continue with the BMP. Designers of such auction programs should consider this possibility and implement measures to minimize crowding out. For example, information reminding landowners of the social and environmental benefits of their auctions may help restore the motivations that induced the voluntary behaviour in the first place.

Chapter 6: Conclusion

While a number of studies have examined internal design features of conservation auctions and their impact on auction success, this study takes research on such auctions in a different direction by examining the social context within which they take place. The experiments described in this study support the hypothesis that social factors are likely to play an important role in the success of conservation auctions. Social capital, leadership, and the pro-social or pro-environmental motivations of landowners all have a significant impact on behaviour within, and the outcomes of, conservation auctions.

These findings play an important role in linking the economic literature on both social capital and leadership to research on conservation auctions. This study incorporated economic theories related to social capital into conservation auction theory, to predict that the bid functions of participants should have an extra term capturing either the utility derived from social approval associated with BMP adoption, or impulses toward reciprocity. This implies that the overall level of BMP adoption in a previous period should have a significant effect on both bids and the auction outcomes resulting from those bids. The experimental results confirm this prediction, with variables representing social capital having a statistically significant impact both on individual bids and on auction outcomes.

The economic literature on leadership provided conflicting predictions regarding the impact of leaders on bidder behaviour and auction outcomes. According to the theory of reciprocity, leaders might have a positive impact by setting a good example for other participants, and/or providing information that was not accessible to others. On the other hand, leaders might have a negative impact by solving the coordination problem relating to collusion, and encouraging others to raise their bids. Indeed, leaders were found to have a multi-dimensional, context-specific impact in the experiment. Unfortunately, their effect was also found to be largely negative in terms of auction outcomes.

Interestingly, this study also draws attention to the important role that social networks may play in conservation auctions. The experiments dealing with social capital found that social norms in favour of BMP adoption had an impact on bids only when participants were part of an existing social group, and had no discernable effect in groups of participants without existing social ties. This finding supports the conceptualization of social capital as a collective, group-level attribute, rather than a characteristic that individuals carry with them between different contexts. Social networks appear to influence the ability of individuals to behave as leaders as well. In groups of participants that knew each other, leaders were able to encourage successful collusion, significantly raising individual bids. In groups without existing social connections, collusion did not appear to be a problem, although leaders did raise average bids by a smaller amount by explaining the auction processes to other participants.

Previous studies have shown that crowding out of internal motivations for pro-social or pro-environmental behaviour can be caused by regulation, fines, and taxes. Crowding out has also been demonstrated in the case of a competitive tender in a public goods game (Reeson and Tisdell 2010). This experiment, which builds on Reeson and Tisdell's work but more closely parallels the incentives found in a conservation auctions, shows that crowding out can also be a problem in this tool for environmental conservation.

This study also presents a new approach to investigating the impact of social factors within an experimental setting. Instead of artificially producing leaders or social connections through the experimental design, these experiments provide channels through which the already existing social characteristics and motivations of individuals can influence their behaviour. As economists become more mindful of the importance of social context for individual behaviour, this approach may prove useful for experimental researchers.

Finally, the findings regarding social capital, leadership and crowding out have important policy implications for designers of conservation auctions. They show that besides the design of the auction itself, the auction's social context is also likely to influence its success.

For example, when selecting an area in which to implement an auction, policy-makers will want to consider the strength of existing social networks in the community, as well as existing norms regarding BMP adoption. In a closely-knit community, a positive norm regarding BMP adoption is likely to improve the outcomes of an auction; conversely, a negative norm towards BMP adoption may have adverse effects. In a community where social networks are not as strong, such norms may be less important.

Auction designers should also be aware of the presence of leaders within communities, particularly those with strong social networks. In these situations, collusion is a real possibility and the design of the auction should include measures to discourage collusion, such as reserve prices. It may also prove helpful to make a priority of convincing these individuals of the importance and benefits of the auction and BMP adoption, in an attempt to prevent them from having a negative influence.

It is important to be aware that the impact of both social capital and leadership are affected by the specific incentive mechanisms through which BMP adoption affects auction participants. In particular, in this experiment, the impacts of both social capital and leadership were different when the "group payments" incentive mechanism was used. In existing groups, social norms had a strong impact when BMP adoption had a direct impact on the individuals participating in the auction, rather than a more distant effect on society at large. Leaders in the group payments treatment appeared to encourage lower bids, in groups that did not know each other; unfortunately, collusion remained a problem in existing groups. These findings imply that policy-makers may want to consider some kind of group incentive when implementing auctions in a community with strong social networks, if measures can be taken to protect against collusion. Or, since group bonuses are likely to increase auction costs, for some BMPs it may be possible to capture these benefits simply by educating landowners about the benefits they can receive from adoption of these BMPs in their area.

The fact that crowding out is shown to be a potential problem for conservation auctions provides a cautionary note to policy-makers. Again, it is

important to consider the existing social context. In a situation where there is a relatively high rate of voluntary BMP adoption, a temporary conservation auction that introduces, then removes monetary incentives may actually prove harmful. Conservation auctions may be better suited to areas where voluntary BMP adoption is uncommon, and used with caution when financial support is only available for a short time.

The research described here is only a starting point for investigation into the impact of social factors on conservation auctions. Several extensions of the research could potentially strengthen the conclusions drawn about these factors, and provide further insight into the mechanisms through which these factors play a role.

Based on the findings regarding the importance of social capital and leadership to both bidder behaviour and auction outcomes, further investigation of these factors would seem to be important. First, conducting experimental studies with landowner participants, rather than students, may provide stronger evidence that social capital and leadership are important to the individuals who actually participate in auctions. Second, it would be interesting to examine how auction design features may interact with the social context to affect bidder behaviour and auction outcomes. Factors such as the information provided to bidders, the process through which bids are submitted, and the payment mechanism may all affect the way that bidders interact with each other and thus have an additional, indirect effect on the decisions that they make. Third, while it may be difficult to isolate the effects of social context in field studies with real auctions because of other confounding factors, investigation of these social factors could at least be incorporated into evaluation and analysis of the performance of such auctions. For example, some real auctions (e.g. Brown et al 2010, Hill et al 2011) have seen relatively low landowner participation rates. Examination of the social norms regarding BMP adoption and auction participation among landowners in the area may shed some light on the reasons for these findings.

The crowding out experiment described in this study follows previous research on crowding out quite closely in terms of the general experimental

strategy used to draw conclusions. One limitation of much of this research, including this study, is that it provides little or no insight into which specific "internal" motivations are crowded out by the various policy mechanisms. Thus, further research could attempt to isolate these internal motivations (norms, reciprocity, altruism etc.) by varying the types of social interaction provided within the experimental design, as has been done in some of the leadership research (Meidinger and Villeval 2002, Potters et al. 2001, 2007). This would improve our understanding of why, precisely, crowding out occurs.

This study has drawn together a number of disparate strands in the economic literature, including both theoretical and empirical work on social capital, leadership, and crowding out and the largely empirically-based, policy-oriented research on conservation auctions. In doing so, it has shown that our understanding of how people respond to practical policy mechanisms like conservation auctions can be significantly enhanced by attention to the importance of social factors in individual and group behaviour and decision-making. It is hoped that this fuller understanding of people's behaviour will provide guidance for better design and implementation of policy mechanisms to improve the environment.

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Appendix A: Questionnaires

For the social capital and leadership experiments, participants were pre-tested for social capital and leadership characteristics using questions from the Social Capital Questionnaire (Bullen and Onyx 1998) and the Multifactor Leadership Questionnaire (Avolio and Bass 1995). All questions from the SCQ used in this study[19], and a selection of questions from the MLQ, are reproduced below.

The social capital and leadership portions of the questionnaire were separately scored by summing the points from each question (in both, higher scores indicate stronger social capital/leadership). Participants were divided into the experimental treatment categories with cutoffs chosen so that the number of individuals in each category was roughly proportional to the number of participants needed for each treatment.

For each question from the SCQ, the mean response and standard deviation are reported in parentheses in the format (mean, standard deviation).

Social Capital Questionnaire, Selected Questions (Bullen and Onyx 1998)

1. Have you ever picked up other people's rubbish in a public place? (2.91, 0.80)

No, never			*Yes, frequently*
1	**2**	**3**	**4**

2. Some say that by helping others you help yourself in the long run. Do you agree? (3.64, 0.56)

No, not much			*Yes, very much*
1	**2**	**3**	**4**

3. Do you help out a local group as a volunteer? (2.87, 1.03)

No, not at all			*Yes, often (at least once a week)*
1	**2**	**3**	**4**

4. Do you feel safe walking down your street after dark? (3.05, 0.94)

No, not much			*Yes, very much*
1	**2**	**3**	**4**

[19] Two questions from the SCQ were omitted due to concerns raised during the ethics review for this project.

5. Do you agree that most people can be trusted? (2.77, 0.79)

No, not much *Yes, very much*

1 **2** **3** **4**

6. If someone's car breaks down outside your house, do you invite them into your home to use the phone? (2.71, 1.02)

No, not at all *Yes, definitely*

1 **2** **3** **4**

7. Can you get help from friends when you need it? (3.63, 0.57)

No, not at all *Yes, definitely*

1 **2** **3** **4**

8. Does your area have a reputation for being a safe place? (3.13, 0.94)

No, not at all *Yes*

1 **2** **3** **4**

9. If you were caring for a child and needed to go out for a while, would you ask a neighbour for help? (2.57, 1.04)

No, not at all *Yes, definitely*

1 **2** **3** **4**

10. Have you visited a neighbour in the past week? (2.01, 1.11)

No, not at all *Yes, frequently*

1 **2** **3** **4**

11. Have you attended a local community event in the past 6 months (e.g., church bazaar, school concert, craft exhibition)? (2.83, 1.14)

No, not at all *Yes, several (at least 3)*

1 **2** **3** **4**

12. Are you an active member of a local organisation or club (e.g., sport, craft, social club)? (2.80, 1.13)

No, not at all *Yes, very active*

1 **2** **3** **4**

13. Does your local community feel like home? (2.88, 0.89)

No, not at all *Yes, definitely*

1 **2** **3** **4**

14. In the past week, how many phone conversations have you had with friends? (3.05, 0.97)

None *Many (at least 6)*

1 **2** **3** **4**

15. How many people did you talk to yesterday? (3.25, 0.78)

None at all *Many (at least 10)*

1 **2** **3** **4**

16. Over the weekend do you have lunch/dinner with other people outside your household? (2.97, 1.00)

No, not much *Yes, nearly always*

1 **2** **3** **4**

17. Do you go outside your local community to visit your family? (2.76, 1.03)

No, not much *Yes, nearly always*

1 **2** **3** **4**

18. When you go shopping in your local area are you likely to run into friends and acquaintances? (2.67, 1.00)

No, not much *Yes, nearly always*

1 **2** **3** **4**

19. If you need information to make a life decision, do you know where to find that information? (3.33, 0.77)

No, not at all *Yes, definitely*

1 **2** **3** **4**

20. In the past 6 months, have you done a favour for a sick neighbour? (1.94, 1.01)

No, not at all *Yes, frequently (at least 5 times)*

1 **2** **3** **4**

21. Are you on a management committee or organising committee for any local group or organisation? (2.02, 1.07)

No, not at all *Yes, several (at least 3)*

1 **2** **3** **4**

22. In the past 3 years, have you ever joined a local community action to deal with an emergency? (1.63, 0.90)

No, not at all *Yes, frequently (at least 5 times)*

1 **2** **3** **4**

23. In the past 3 years have you ever taken part in a local community project or working bee? (2.07, 1.05)

No, not at all *Yes, very much*

1 **2** **3** **4**

24. Have you ever been part of a project to organise a new service in your area (e.g., youth club, scout hall, child care, recreation for disabled)? (1.90, 1.05)

No, not at all *Yes, several times(at least 3)*

1 **2** **3** **4**

25. If you disagree with what everyone else agreed on, would you feel free to speak out? (3.04, 0.83)

No, not at all *Yes, definitely*

1 **2** **3** **4**

26. If you have a dispute with your neighbours (e.g., over fences or dogs), are you willing to seek mediation? (3.12, 0.81)

No, not at all *Yes, definitely*

1 2 3 4

27. Do you think that multiculturalism makes life in your area better? (3.41, 0.81)

No, not at all *Yes, definitely*

1 2 3 4

28. Do you enjoy living among people of different life styles? (3.36, 0.79)

No, not at all *Yes, definitely*

1 2 3 4

29. If a stranger, someone different, moves into your street, would they be accepted by the neighbours? (3.21, 0.75)

No, not easily *Yes, definitely*

1 2 3 4

Multifactor Leadership Questionnaire, Selected Questions (Avolio and Bass 1995)

In total, there were 28 questions in this part of the questionnaire, chosen from the MLQ. Permission was given by the survey publishers to reproduce five sample questions only. All questions were answered using a five-point rating scale, where 0 indicates “not at all” and 4 indicates “frequently, if not always.”

1. I provide others with assistance in exchange for their efforts

2. I re-examine critical assumptions to question whether they are appropriate

3. I focus attention on irregularities, mistakes, exceptions, and deviations from standards

4. I talk about my most important values and beliefs

5. I seek differing perspectives when solving problems

Appendix B: Social Capital & Leadership Experimental Instructions

Prior to the social capital and leadership experiments, all participants were given the following instructions in the form of a PowerPoint presentation which they read through individually. One additional slide was included in the Group Payments treatment. The instructions also included screenshots from the experimental software and a brief explanation about how to use it. The experimenter then invited and answered questions regarding the experimental procedure.

Instructions Given To All Participants

Introduction to the Experiment

- Today's experiment is meant to study economic decision-making.
- You will represent a business owner, and will have to make decisions about how to manage your business.
- Your business earns profit that benefits yourself. In addition, you have the ability to contribute (in terms of time, money, and/or goods) to social causes that benefit your local community. However, these contributions are costly for you. As a business owner, you must decide whether or not to contribute to these social causes.

How It Works

- At the beginning of each period, you are assumed to manage your business in a conventional way that generates profits of $15 for you that period.
- You may choose to contribute to a social cause that benefits your local community. This contribution costs you money, which is deducted from your profits. However, the government is willing to provide some compensation to businesses that make these contributions.
- In real life, businesses that contribute to social causes provide real benefits to society. To represent this in the experiment, the experimenters will

make a monetary donation on your behalf to the U of A Campus Food Bank.

- There will be several periods during this experiment. At the end of the experiment, ***one*** period will be randomly chosen as the binding period. You will receive your earnings from this period, in cash, plus your $5 show-up fee. If you chose to contribute to a social cause during this period, the experimenters will make a **real** monetary donation on your behalf to the Campus Food Bank.
- The earnings and contributions from the other periods will not result in real payments or donations. However, remember that you do not know ahead of time which period will be binding – the decision you make during *any* round could be the one that results in real payments and donations.

Your Decision

- Each period, you have to choose whether or not to contribute to a social cause that benefits your local community.
- The cost of making this contribution will be displayed on your computer screen. If you decide to contribute, this cost will be deducted from your $15 profit for that period.
- The social benefit of this contribution will also be displayed on your computer screen (this is the value to the community of your contribution of time, money, and/or goods). If you decide to contribute, the experimenters will make a monetary donation, equal to the social benefit, to the Campus Food Bank on your behalf.
- The costs and social benefits are different for everyone, and will change from time to time during the experiment.

Government Compensation

- The government recognizes that contributing to social causes is costly to businesses. To increase the amount of these contributions, the government has designed a program to compensate businesses for these costs (similar

to the tax refunds that individuals receive for making charitable donations).

- Each period, you will be invited to submit an *offer* which specifies the amount of compensation that you would like to receive in return for contributing to the social cause.
- The government evaluates each offer by calculating the following:

$$\frac{\text{compensation requested}}{\text{social benefit}}$$

- The lower this ratio, the better the offer, since this allows more businesses to be compensated for making contributions. The government will accept the best offers first, until the fixed budget that is allocated to this program has been used up. The rest of the offers will be rejected.
- If your offer is accepted, you will receive the compensation you asked for, in addition to your regular business profit ($15). You will be required to contribute to the social cause, and the cost of doing this will be deducted from your earnings.
- If your offer is rejected, you will not receive any compensation, but will still receive your regular business profit ($15). You can still contribute to the social cause if you wish, and the cost will be deducted from your $15 profit.
- You do not have to submit an offer. If you do not, you still earn your regular business profit ($15). You can still contribute to the social cause if you wish, and the cost will be deducted from your $15 profit.

A Note

- The most you can be paid for this experiment is $30, plus your $5 show-up fee. Even if your net earnings are greater than $30 for the period that is randomly chosen to be binding, $30 is the most you will be paid (plus the $5 show-up fee).

Other Participants

- Your computer screen will display information about the decisions made by each of the other participants. For example, it may show that

Participant 1 submitted an offer in the last period and that he/she made a contribution to the social cause.

- However, no one knows anyone else's participant number. So, no one will know which person in the room is Participant 1. You do not have to tell anyone your participant number or anything about your own decisions, even if they ask.
- Throughout the experiment, you can communicate with the other participants, if you wish.
- There will be a chat box on your computer screen. You can chat anonymously with all other participants through this box. The chat box will disappear whenever you have made a decision and are waiting for others to make their decisions, so don't click OK on the decision screen until you are done chatting.
- You may also feel free to talk with the other participants at any time during the experiment. Please do not leave your computer, just talk from your place.

Additional Instructions for Group Payments Treatment

This slide was included after the "Government Compensation" slide.

Group Payments

- The government would like to achieve at least a minimum amount ($72.42) of social contributions.
- Each period, if this target is reached, the government will pay a bonus payment of $1 each to all business owners. Everyone will receive the bonus, regardless of whether or not they submitted a bid or whether their bid was successful.
- If this target is not reached, no one will receive a bonus payment. However, successful bidders will still receive their government compensation.
- Depending on your costs and benefits, you may not be able to participate successfully in the compensation program. However, everyone has the

opportunity to contribute to reaching the target by encouraging others to make appropriate offers and contributions.

Appendix C: Crowding Out Experimental Instructions

Prior to the crowding out experiments, all participants, in both treatments, were given the following instructions in the form of a PowerPoint presentation which they read through individually. The instructions also included screenshots from the experimental software and a brief explanation about how to use it. After participants had read through the instructions, the experimenter summarized the instructions verbally and answered questions regarding the experimental procedure. During the experiment, participants in the auction treatment also read through the additional instructions presented below, and were invited to ask questions if they needed further clarification. Participants in the control treatment received no additional instructions during the experiment.

Instructions Given To All Participants

Introduction to the Experiment

- Today's experiment is meant to study individual decisions about allocating resources between contributing to one's own well-being and contributing to public goods. Public goods are goods that benefit many people in a society. Some examples are roads, a clean environment, and fire safety services.
- You will represent a farmer, who has to make decisions about land management. You can manage your farm to provide benefits that support you and your household, or you can adopt environmentally friendly management practices that provide environmental benefits to society. You can also choose a combination of these objectives.

How it Works

- At the beginning of each round, you are assumed to manage your farm in a conventional way that generates benefits to you only. If you do not change

your management practices, you earn $2 (to be paid in cash at the end of the experiment).

- However, each round you have the opportunity to contribute environmental benefits to society by adopting environmentally friendly management practices.
- Adopting these practices costs you money, which is deducted from the $2 that you would otherwise earn. However, adopting these practices also benefits the environment. To represent this, the experimenters will make a **real** monetary donation on your behalf to a charity that works to improve the environment in Canada.

Your Decision

- Each round, you can choose to adopt a certain level of environmentally friendly management practices. In real life, these practices vary from farm to farm so we will not tell you exactly what they are. They might include things like nutrient management, restoration of wetlands, and crop rotation. Farmers can adopt some or all of these practices, depending how much they are willing to spend.
- In this experiment, you can choose to adopt 0%, 25%, 50%, 75%, or 100% of the practices that will work on your farm.

Costs of Adopting

- Your cost of adopting these environmentally friendly practices *will change from round to round.* Each round, you will be told the per unit cost for adopting each 25% increment. For example, your per unit cost in one round might be $0.20. This means your options would be as follows:

I adopt	**Adoption cost**
0%	$0.00
25%	$0.20
50%	$0.40
75%	$0.60
100%	$0.80

- Each round, the cost of your chosen level of adoption will be deducted from the $2 that you would receive for conventional farming.

Benefits of Adopting

- The environmental benefit of adopting these practices will be reflected by **real** donations to an environmental charity, which the experimenters will make on your behalf, as follows:

I adopt	**Charity receives**
0%	$0.00
25%	$0.50
50%	$1.00
75%	$1.50
100%	$2.00

- These environmental benefits are the same in every round of the experiment, and are the same for everyone. They do not change, even if your adoption cost changes.

Some Notes

- Each round, you cannot spend more than your farm income ($2) on adopting environmentally friendly practices. The program will let you know if you are trying to spend too much.
- The program will keep track of your earnings each round (farm income minus adoption costs) in an experimental bank account. At the end of the experiment, you will be paid the money in this account, in cash.
- The most you can be paid for this experiment is $30, plus your $5 show-up fee. Even if your bank account contains more than $30, $30 is the most you will be paid (plus the $5 show-up fee).

Other Participants

- Your computer screen will display information about the management decisions made by each of the other participants. For example, it may show that Participant 1 chose an adoption level of 50% in the last round.
- However, no one knows anyone else's participant number. So, even though everyone knows that Participant 1 chose 50%, no one will know which person in the room is Participant 1. You do not have to tell anyone your participant number or anything about your own decisions, even if they ask.

- Throughout the experiment, you can communicate with the other participants, if you wish.
- There will be a chat box on your computer screen. You can chat anonymously with all other participants through this box. The chat box will disappear whenever you have made a decision and are waiting for others to make their decisions, so don't click OK on the decision screen until you are done chatting.
- You may also feel free to talk with the other participants at any time during the experiment. Please do not leave your computer, just talk from your place.

***A** Note*

- There will be several rounds in this experiment. We may or may not change the procedures later in the experiment. The researcher will let you know!

Instructions after Round Four of Auction Treatment

We are now going to change the experimental procedure!

Starting now, the government is offering compensation to farmers who adopt environmentally friendly practices that benefit society.

Before you make a decision about an adoption level, you will be asked to provide an "offer." This offer specifies the level of adoption that you will commit to, and the PER-UNIT amount of compensation that you would like to receive for each 25% increment of adoption. For example, you might specify an adoption level of 50% and a per-unit compensation level of $0.30. If your offer was accepted, you would receive $0.60 in compensation ($0.30 for the first 25% increment and $0.30 for the second 25% increment). This compensation is in addition to your regular farm earnings.

There is a certain experimental budget that will be used to pay participants for their offers. The offers with the LOWEST per-unit compensation will be accepted first, until the budget is used up. The rest of the offers will be rejected.

If your offer is accepted, you will receive the compensation you asked for, in addition to your regular farm earnings ($2 minus adoption costs). You will be required to provide at least the level of adoption that you specified in your offer. You can provide a higher adoption level if you wish, but you will not receive compensation for the extra adoption levels.

If your offer is rejected, you will not receive any payment on top of your regular farm earnings. You can still choose to adopt environmentally friendly practices if you wish, and the costs will be deducted from your $2 farm earnings as usual.

If you don’t want to submit an offer, just click on the "do not participate" button. You can still choose to adopt environmentally friendly practices if you wish, and the costs will be deducted from your $2 farm earnings as usual.

All the other rules regarding costs, donations, etc. remain the same as before.

Instructions after Round Eight of Auction Treatment

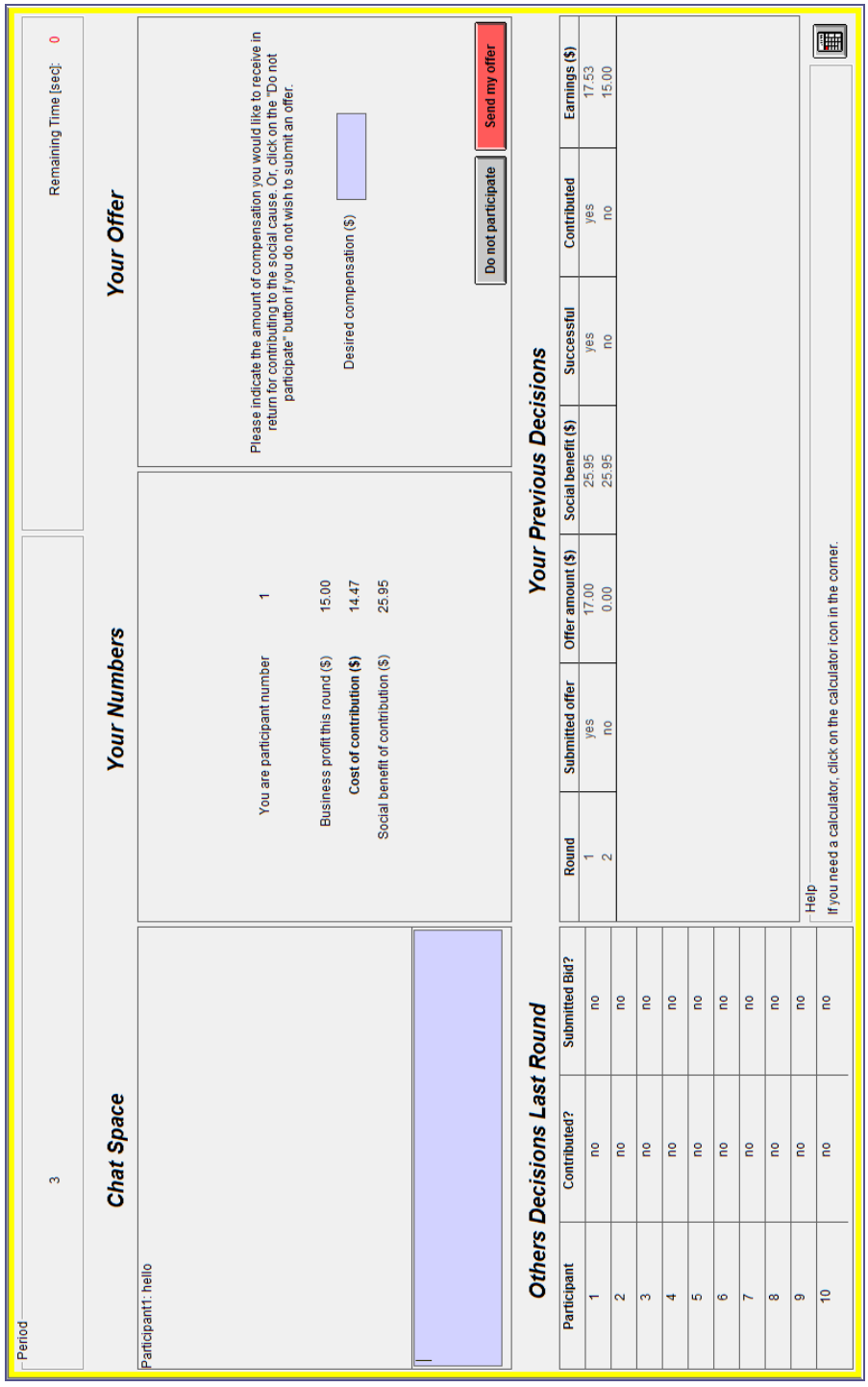
We are now going to change the experimental procedure!

Starting now, we are going to go back to the original procedure. The government will no longer provide compensation for adopting environmentally friendly practices.

You will no longer have the opportunity to provide an offer. You will simply be asked, each round, to make a decision about adopting environmentally friendly practices. As before, the costs will be deducted from the $2 you could earn from conventional farming.

Appendix D: Screenshots from Experiments

Social Capital and Leadership Experiments Decision Screen (sample)



Crowding Out Experiments Decision Screen (sample)