8-2013

Essays on Forward Trading, Environmental Quality and Investor Behavior, and the WTA-WTP Disparity

Jens Schubert
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To the Graduate Council:

I am submitting herewith a dissertation written by Jens Schubert entitled "Essays on Forward Trading, Environmental Quality and Investor Behavior, and the WTA-WTP Disparity." I have examined the final electronic copy of this dissertation for form and content and recommend that it be accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy, with a major in Economics.

Christian Vossler, Major Professor

We have read this dissertation and recommend its acceptance:

William Neilson, Andy Puckett, Michael K. Price

Accepted for the Council:

<u>Carolyn R. Hodges</u>

Vice Provost and Dean of the Graduate School

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Essays on Forward Trading, Environmental Quality and Investor Behavior, and the WTA-WTP Disparity

A Dissertation Presented for the

Doctor of Philosophy

Degree

The University of Tennessee, Knoxville

Jens Schubert

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"When a truth is necessary, the reason for it can be found by analysis, that is, by resolving it into simpler ideas and truths until the primary ones are reached." - Gottfried Wilhelm von Leibniz

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Abstract

This dissertation consists of three essays that study (i) collusion in forward markets, (ii) investor behavior in response to ecological disasters, and (iii) the willingness to accept - willingness to pay disparity in the presence of uncertainty.

Chapter 1 reports the results of a laboratory experiment that examines the strategic effect of forward contracts on market power in infinitely repeated duopolies. Two competing effects motivate the experimental design. Allaz and Vila (1993) argue that forward markets act like additional competitors in that they increase quantity competition among firms. Conversely, Liski and Montero (2006) argue that forward contracting can facilitate collusive outcomes by enabling firms to soften competition. The experiment provides a first simultaneous test of these rival effects. Contrary to previous experimental studies, the results do not support the quantity-competition effect. Further, the findings provide evidence in support of the collusive hypothesis.

Chapter 2 analyzes investor behavior in response to ecological disasters. Specifically, I test for the presence of "green" preferences in stock markets using variation in abnormal returns of publicly traded companies that differ in their environmental footprint. I employ Newsweek's detailed green rankings of the 500 largest U.S. firms as an identification strategy and I find that cumulative abnormal returns following an ecological disaster significantly differ based on companies' environmental performance scores.

Chapter 3 reports the results of a laboratory experiment that tests the robustness of the willingness to accept - willingness to pay disparity for private good and public good lotteries in the presence of uncertainty. Using an incentive compatible elicitation mechanism, the experiment explores the existence of the disparity and its interdependence with uncertainty and the income effect. The findings suggest that the disparity persists for both private and public goods for monetary lotteries. While there is significant evidence for social preferences in the public good setting, the disparity is even stronger than in the private good setting.

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Chapter 1

The Impact of Forward Trading on Tacit Collusion: Experimental Evidence

1.1 Introduction

Antitrust authorities and researchers have a profound interest in identifying and understanding the factors that determine the likelihood of collusion. There is extensive theoretical and empirical work that focuses on the determinants of firms' coordinated efforts to achieve profits in excess of the competitive outcome. Most empirical evidence comes from experiments as relevant field data is difficult to obtain and identification of specific factors can be challenging due to interactions and unobservables. Controlled laboratory experiments, however, allow targeted tests in market environments that satisfy the assumptions of the underlying model of interest. This article studies the effect of forward contracting on tacit collusion in laboratory Cournot duopolies.

Historically, forward contracts have played an important role in commodity markets and more recently in financial asset markets. They have also become increasingly important in electricity wholesale markets. Forward contracting is a prevalent instrument in hedging risk: Forward contracts allow buyers and sellers to potentially offset unfavorable price movements in the spot market by shifting risk to less risk-averse market participants[1].

Theoretical work, however, suggests that even in the absence of risk and uncertainty, forward markets create strategic incentives for firms. There are two competing strands in the theoretical literature on the strategic effects of forward markets in a setting where firms compete over quantity. One strand argues that a forward market acts like additional competitors in that it increases quantity competition among firms (Allaz, 1992; Allaz and Vila, 1993; Bushnell, 2007). The underlying intuition is that quantity-setting firms will sell some of their production forward to improve their position relative to competitors in the spot market. In the spot market, firms will then compete over the residual demand. The forward market therefore creates a prisoner's dilemma where each firm has an incentive to sell forward to improve its position relative to its competitor(s) in the spot market (Stackelberg leader advantage). Nonetheless, firms would be better off jointly by not trading forward. As a result, each firm produces more than in the absence of forward markets, total output increases, market price decreases and therefore market efficiency increases. Following the Western U.S. energy crisis of 2000 and 2001, this pro-competitive prediction led to suggestions to remove restrictions on forward contracts with the goal of limiting

[1]See Wolak (2000); Bessembinder and Lemmon (2002) for a discussion of forward contracts as optimal hedging instruments in wholesale electricity markets.

the ability of electricity generators to exercise market power (Joskow, 2001).

The other strand of theoretical work challenges this market efficiency enhancing prediction. The pro-competitive hypothesis results from a finite horizon assumption. In an infinitely repeated setting, however, the existence of forward markets can increase the likelihood of collusive outcomes. According to the Folk (Friedman) theorem, the collusive outcome is one of many equilbria in an infinitely repeated setting. Liski and Montero (2006) argue that, in infinitely repeated oligopolies, forward markets give firms the ability to maintain the tacitly collusive outcome more so than in the absence of forward markets. In particular, the range of discount factors that support the collusive equilibrium is wider when firms repeatedly interact both in forward and spot markets. Two effects drive this result. First, the gains from deviating from the collusive path are never greater than the gains in an infinitely repeated oligopoly without forward markets. However, importantly, the greater a firm's forward position, the lower its incentive to defect in the spot market. Second, the subgame perfect sanctioning strategy (Allaz and Vila equilibrium) is more costly than in the absence of forward markets (standard Cournot equilibrium). Other theoretical work also suggests that forward markets yield strategic motives that do not mitigate firms' market power (Ferreira, 2003; Mahenc and Meunier, 2003; Mahenc and Salanié, 2004; Murphy and Smeers, 2010).

One motivating example in the literature are restructured wholesale electricity markets. Most wholesale electricity markets are characterized by a few firms producing a homogeneous good that cannot be stored economically at the large scale; and these firms repeatedly interact in both forward and spot markets. However, empirical work on the effects of forward contracts in wholesale electricity markets is limited and the findings are mixed (Bessembinder and Lemmon, 2002; Shawky et al., 2003).

The focus of this article is to investigate whether forward sales yield strategic effects in a setting where the same firms repeatedly interact in Cournot oligopolies. In particular, I test the collusive hypothesis of Liski and Montero against the pro-competitive hypothesis of Allaz and Vila in a controlled laboratory experiment. Previous experimental studies on the strategic effect of forward markets report results that support the pro-competitive prediction (Le Coq and Orzen, 2006; Van Koten and Ortmann, 2011; Brandts et al., 2008; Ferreira et al., 2010). These studies do not find evidence of collusive outcomes. However, the experimental designs in these studies do not support the collusive prediction[2]. In this article, I report the results of an experimental design that supports both collusive and pro-competitive hypotheses in indefinitely repeated duopolies[3]. The design differs from previous studies in that it strictly imposes forward-spot price parity to eliminate any price uncertainty effects and to allow for multiple collusive strategies that involve forward trading. Forward-spot price parity implies that the forward price is equal to the spot price. Further, the design restricts subjects' quantity choices to a discrete choice set, which reflects different pure strategies in the Cournot stage-game and allows me to constrast strategy choices by treatment.

I compare the market outcomes of a two-phase duopoly with forward trading to the results of a standard, single-phase duopoly. Specifically, I examine differences in collusive outcomes between these two treatments. I investigate stage-game outcomes in the spot market phase conditional on forward phase outcomes to test for differences in strategic play between colluding and non-colluding firms. To compare the compet-

[2]The designs use a forward market pricing rule that (1) eliminates collusive strategies that involve forward sales; and that (2) introduces price uncertainty between forward and spot market.

[3]Note that repeated play of the Allaz and Vila stage-game strategy is one of many subgame perfect equilibrium strategies in an infinitely repeated setting.

itive effect of market entry to the effect of forward markets, I report the differences in market efficiency between a three-firm oligopoly and the two-phase duopoly.

The main result of the experiment is that, contrary to previous experimental findings, introducing a forward market in a duopoly may not increase market efficiency. The findings in the standard duopoly treatment are not different from the findings of Le Coq and Orzen (2006). Nevertheless, market efficiency is signficantly lower in the forward-phase duopoly treatment. The pro-competitive hypothesis predicts that the effect of a forward market is equivalent to squaring the number of firms. However, I find that one additional competitor significantly limits market power in a duopoly whereas a forward market does not. Further, I provide evidence that allowing firms to trade forward can facilitate collusive outcomes.

The organization of the remainder of the article is as follows. Section 1.2 presents the predictions of the pro-competitive and collusive theories and derives the hypotheses which guide the experimental design. Section 1.3 describes the experimental design and procedures. Section 1.4 presents the results of the article, and Section 1.5 discusses the main findings.

1.2 Theoretical Framework and Hypotheses

First, I derive the pro-competitive predictions of the stage-game and then contrast them to the collusive predictions of the infinitely repeated game. Notice that, according to the Folk theorem, repeated play of the stage-game equilibrium strategy is a subgame perfect equilibrium strategy in the indefinitely repeated game. In the following derivation, I only consider a single forward market opening prior to the spot market (for a detailed derivation with multiple forward market openings, see Allaz and Vila, 1993; Ferreira, 2003).

1.2.1 Competitive Framework

1.2.1.1 Standard Cournot Game

First, consider a single phase Cournot game with J firms that compete over quantity. Without loss of generality, assume symmetric firms with zero production cost. For simplicity, let the inverse demand function be given by

$$p\left(q\right) = \alpha - \sum_{j=1}^{J} q_j, \tag{1.2.1}$$

where q_j denotes firm j's output. The single period, unique Nash equilibrium is given by

$$q_j^c = \frac{\alpha}{J+1};\ \pi_j^c = \frac{\alpha^2}{\left(J+1\right)^2};\ \forall j;\ p^c = \frac{\alpha}{J+1}, \tag{1.2.2}$$

where π_j denotes firm j's profits. Backward induction implies that the one-shot game predictions hold in a finitely repeated game.

1.2.1.2 Two-Phase Cournot Game

Now consider a two-phase Cournot game in which a forward market is followed by a standard Cournot game spot market. The good is physically bought and sold in the spot market. In the first phase (forward market), firms can sell some or all of their production for delivery in the second phase (spot market). At the end of the first phase, firms observe the forward market outcome. In the second phase, firms compete in quantity over the residual demand. At the end of the second phase, firms observe the spot market production and total production of their competitor(s), the market price, p, and profit π_j[4].

The existence of arbitrage traders in the market will yield forward-spot price parity, $p^f = p^s = p(q)$ (where p^f (p^s) denote the forward-phase (spot-phase) price, respectively). Arbitrage traders will compete in prices over firms' short forward positions and will try to sell them at a profit to buyers in the spot market. In equilibrium, any price differences between the two phases will disappear. Another way to think about forward-spot price parity is that buyers have perfect foresight and are therefore indifferent between buying in the forward or spot market.

The game can be solved using backward induction. Let f (s) denote total units sold in the first (second) phase, respectively. The model assumes that firms treat their first-phase profits as being unaffected by their second-phase production decisions. Given forward positions, firm j's profit maximization problem in the spot market game can be written as

$$\max_{s_j} \; p\left(s_j, s_{\neg j}, f\right) s_j \text{ for } j = 1, \ldots, J, \tag{1.2.3}$$

with corresponding first order condition

$$0 = p\left(\cdot\right) + \frac{\partial p\left(\cdot\right)}{\partial s_j} s_j \;\forall j. \tag{1.2.4}$$

With an inverse demand function as given in equation 1.2.1, the first order condition is

$$0 = \alpha - f - \sum_{k=1}^{J} s_k - s_j \;\forall j. \tag{1.2.5}$$

Simultaneously solving the J best response functions gives firm j's optimal second

[4]For a detailed derivation of the two-phase equilibrium, see Allaz and Vila, 1993 and Bushnell, 2007.

phase production:

$$s_j(f) = \frac{\alpha - f}{J+1} \ \forall j, \tag{1.2.6}$$

which is a best response to any arbitrary level of forward sales commitment. To obtain the first phase equilibrium, the second phase best response functions are nested in the first phase objective function:

$$\max_{f_j} \ p\left(f_j, \sum_{k \neq j}^{J} f_k, \sum_{k=1}^{J} s_k(f)\right)(f_j + s_j(f)) \ \forall j, \tag{1.2.7}$$

with corresponding first order condition

$$\begin{aligned} 0 &= p(\cdot)\left(1 + \frac{\partial s_j}{\partial f_j}\right) + \frac{\partial p}{\partial q}\left(1 + \sum_{k=1}^{J} \frac{\partial s_k}{\partial f_j}\right)(f_j + s_j) \ \forall j & (1.2.8) \\ &= \frac{J-1}{J+1}(\alpha - f) - f_j. & (1.2.9) \end{aligned}$$

Simultaneously solving the J first order conditions and imposing symmetry gives

$$f_j = \frac{J-1}{J^2+1}\alpha \ \forall j. \tag{1.2.10}$$

The two-phase Cournot equilibrium can be summarized as

$$f_j^{fs} = \frac{J-1}{J^2+1}\alpha; \ s_j^{fs} = \frac{1}{J^2+1}\alpha; \ q_j^{fs} = \frac{J}{J^2+1}\alpha; \ \pi_j^{fs} = \frac{J}{(J^2+1)^2}\alpha^2 \ \forall j, \tag{1.2.11}$$

with equilibrium price

$$p^{fs} = \frac{\alpha}{J^2+1}. \tag{1.2.12}$$

Note that the Cournot equilibrium output of a J-firm, two-phase oligopoly equals the output of a J^2-firm, single-phase oligopoly: $q^{fs}(J) = q^c(J^2)$. To summarize, in a finitely repeated setting, the existence of a single forward market increases quantity competition between firms which increases market efficiency. The following two hypotheses characterize the predictions of the finitely repeated two-phase game:

Hypothesis 1.1. Oligopoly markets with a forward market phase yield higher output (lower prices) on average than oligopoly markets with only a spot market phase.

Hypothesis 1.2. The market outcome (total output, price, and profit) of a J-firm, two-phase oligopoly is equivalent to the market outcome of a J^2-firm, single phase oligopoly.

1.2.2 Tacit Collusion

Next, consider an infinitely repeated Cournot game where the same firms compete repeatedly with each other. In general, players can maintain cooperative subgame perfect equilibria if they are concerned about future profits and possible future punishments. Friedman (1971) shows that for sufficiently high discount rates δ, all firms jointly producing the monopoly quantity is a subgame perfect equilibrium strategy. Assume that firms can perfectly observe market outcomes and other firms' actions. I assume that when firms play the cooperative subgame strategy, they split the monopoly output equally. The stage-game collusive outcome can be summarized as

$$q_j^{tc} = \frac{\alpha}{2J};\ \pi_j^{tc} = \frac{\alpha^2}{4J};\ p^{tc} = \frac{\alpha}{2}\ \forall j. \tag{1.2.13}$$

Comparison of the different equilibrium profit predictions yields $\pi_j^{fs} < \pi_j^c < \pi_j^{tc}$.

In deriving the cooperative, subgame perfect equilibrium predictions, I assume that firms will play the cooperative subgame strategy until a firm deviates. Once one firm deviates from the cooperative path, firms will play the stage-game Nash equilibrium strategy thereafter[5].

1.2.2.1 Standard Cournot Game

In the single phase Cournot game, firm j's one-period incentive to deviate from the collusive strategy (deviating) is

$$\max_{q_j} \left(\alpha - (J-1)\frac{\alpha}{2J} - q_j\right) q_j. \tag{1.2.14}$$

Firm j's production and profit and the resulting market price are:

$$q_j^d = \frac{(J+1)}{4J}\alpha;\ \pi_j^d = \frac{(J+1)^2}{16J^2}\alpha^2;\ p^d = \frac{J+1}{4J}\alpha. \tag{1.2.15}$$

The cooperative strategy q^{tc} will be a subgame perfect equilibrium strategy, if the following condition holds:

$$\pi_j^d + \frac{\delta}{(1-\delta)}\pi_j^c < \frac{1}{(1-\delta)}\pi_j^{tc},\ \delta \in [0,\, 1]\,. \tag{1.2.16}$$

The implied critical discount factor for the existence of the subgame perfect equilibrium can be calculated as

$$\frac{(J^2-1)^2}{(J+1)^4 - 16J^2} < \underline{\delta}\,(J)\,. \tag{1.2.17}$$

[5] This is one of many cooperative subgame perfect equilibrium strategies.

1.2.2.2 Two-Phase Cournot Game

In the following derivation, I extend Liski and Montero's framework to an oligopolistic setting with J firms. For simplicity, I restrict firms' positions in the forward market to short positions only (see Liski and Montero (2006) for details on firms' holding long positions in the forward market). In the two-phase Cournot game, several collusive strategies support the subgame perfect equilibrium. In the cooperative subgame, the following strategy supports the collusive subgame outcome: firm j sells $f_j^{tc} = \lambda_j \cdot q_j^{tc}$, $\lambda_j \in [0, 1]$ units in the forward market (first phase) and $s_j^{tc} = (1 - \lambda_j) \cdot q_j^{tc}$ units in the spot market (second phase). Notice that firms' forward positions do not have to be symmetric ($\lambda_i \neq \lambda_j$) in order for the collusive subgame perfect equilibrium to exist. This is an extension of Liski and Montero's model. Firms' production decisions in the spot market do not affect their forward market profits. This implies that if a firm has a short forward position, its incentives to deviate from the collusive path are smaller in the spot-phase stage-game. As a result, the gains of deviating from the collusive path will never be greater than the profit from deviating in the single phase stage-game. Further, deviation is more costly in the two-phase game as the sanctioning path is the two-phase stage-game Cournot equilibrium strategy. These two effects result in a strictly lower critical discount factor that supports the collusive outcome. Firm j's one-period incentive to deviate from the collusive strategy in the spot market is[6]

$$\max_{s_j} \left(\alpha - (J-1)\frac{\alpha}{2J} - \lambda_j \frac{\alpha}{2J} - s_j \right) s_j, \tag{1.2.18}$$

[6] Liski and Montero (2006) show that it is never profitable to deviate in the forward market.

where $\lambda_j \alpha / 2J = \lambda_j q_j^{tc}$ denotes firm j's forward sales expressed in terms of the collusive amount. Firm j's production and profit and the resulting market price are:

$$f_j = \lambda_j \frac{\alpha}{2J};\ s_j^d = \frac{(J+1-\lambda_j)}{4J}\alpha;\ \widetilde{\pi}_j^d = \pi_j^d - \frac{\lambda_j^2}{16J^2}\alpha^2;\ \widetilde{p}^d = \frac{(J+1-\lambda_j)}{4J}\alpha. \quad (1.2.19)$$

Note that the one period profit from cheating in the two-phase game is always less than or equal to the single phase deviating profit. Strategy $\left\{s_j^{tc},\ f_j^{tc}\right\}$ denotes a subgame perfect equilibrium strategy if the following inequality is satisfied

$$\widetilde{\pi}_j^d + \frac{\delta}{(1-\delta)}\pi_j^{fs} < \frac{1}{(1-\delta)}\pi_j^{tc}. \quad (1.2.20)$$

The left-hand side in equation 1.2.20 is strictly less than the left-hand side in equation 1.2.16. The critical discount factor is therefore strictly lower than the critical discount factor in the single phase game:

$$\frac{\left[(J+1)^2 - \lambda_j^2 - 4J\right](J^2+1)^2}{\left((J+1)^2 - \lambda_j^2\right)(J^2+1)^2 - 16J^3} < \widetilde{\underline{\delta}}(\lambda_j, J) < \underline{\delta}(J),\ \forall \lambda_j \in [0,\ 1]. \quad (1.2.21)$$

Note that $\widetilde{\underline{\delta}}(\lambda_j, J)$ is decreasing in λ_j. Table 1.1 summarizes the subgame equilibria predictions.

The following main hypotheses guide the experimental design. These hypotheses reflect the cooperative subgame perfect equilibrium predictions in the infinitely repeated, two-phase Cournot game.

Hypothesis 1.3. In an indefinitely repeated setting, two-phase oligopoly markets yield lower output (higher prices) on average than single phase oligopolies.

Hypothesis 1.4. Firms can sustain the cooperative subgame equilibrium across both phases (forward and spot market) in indefinitely-repeated oligopolies.

Hypothesis 1.5. In indefinitely repeated two-phase oligopolies, firms that sell forward are less likely to defect than firms that have no forward sales position.

Table 1.1: Theoretical Market Outcome Predictions

	f	s	q	p	π_j
Single Phase Subgame	-	$\frac{J}{J+1}\alpha$	$\frac{J}{J+1}\alpha$	$\frac{1}{J+1}\alpha$	$\frac{1}{(J+1)^2}\alpha^2$
Two-Phase Subgame	$\frac{J(J-1)}{J^2+1}\alpha$	$\frac{J}{J^2+1}\alpha$	$\frac{J^2}{J^2+1}\alpha$	$\frac{1}{J^2+1}\alpha$	$\frac{J}{(J^2+1)^2}\alpha^2$
Cooperative Subgame	$\frac{\lambda}{2}\alpha$	$\frac{(1-\lambda)}{2}\alpha$	$\frac{1}{2}\alpha$	$\frac{1}{2}\alpha$	$\frac{1}{4J}\alpha^2$

Note: In the single phase stage-game: $\lambda = 0$. In the two-phase stage-game: $\lambda \in [0, 1]$.

1.3 Experimental Design

The objective of the experimental design is to test the strategic effect of forward sales in an infinitely repeated setting. In order to test for the existence of cooperative subgame equilibria, it is important to create a market environment in the laboratory that gives the predicted collusive equilibria the best chance of occurrence. The following main findings from previous oligopoly experiments contributed to my design (see Engel, 2007 for a comprehensive meta-analysis of oligopoly experiments). First, the larger the number of firms, the smaller the observed degree of collusion (see also Huck et al., 2004). Second, experienced subjects tend to collude more than inexperienced subjects, i.e. learning plays an important role (Huck et al., 1999). Third, the better subjects are informed, the more likely they play a cooperative strategy. Lastly, if subjects play against human buyers, "collusion rates plummet" (Engel, 2007).

My experiment compares a standard duopoly (C2 treatment) to a two-phase duopoly with a single forward and a single spot market phase (FS2 treatment). A third, standard three-firm oligopoly treatment (C3 treatment) allows us to analyze differences between the effect of adding one additional competitor to the effect of a single forward market. Adding one additional competitor serves as a lower bound on the effect of increased competiton from additional firms.

1.3.1 Strategy Design

The main design challenge is to implement forward-spot price parity. The underlying theoretical models assume that demand has perfect foresight. However, in the laboratory, it is impossible to perfectly predict the decisions that subjects make in a stage-game. Previous experimental studies that test the pro-competitive prediction (Le Coq and Orzen, 2006; Ferreira et al., 2010; Van Koten and Ortmann, 2011) use a pricing rule which dictates the forward price to equal the spot price if and only if all firms play the pro-competitive strategy. This pricing rule introduces price uncertainty and it eliminates all cooperative subgame perfect strategies in the forward market as the calculated forward price is always less than the collusive price. Brandts et al., 2008 let human buyers compete over firms' forward market positions in a Bertrand game; however, this does not give the collusive hypothesis a fair chance as it significantly reduces the likelihood of collusive outcomes (Engel, 2007).

My design automates demand using a computer program. I implement forward-spot price parity by restricting subjects' quantity choices to a discrete choice set. The market price is not determined until after the end of the spot phase. This implies that subjects do not observe their forward profits before making their spot phase decisions[7].

[7]Subjects only observe the forward quantity commitments.

Instead, the quantity choices in the spot phase of the stage-game are calculated as if the spot phase choices do not affect the profits in the forward market. The set of limited strategies also decreases unintended effects of inexperienced subjects and increases the likelihood of collusive outcomes (Holt, 1995).

In the forward phase of the FS2 treatment, subjects have the following two choices: either selling zero units or selling the stage-game equilibrium forward quantity as predicted by the pro-competitive theory. Notice that the forward quantity is less than the collusive amount, which admits a collusive strategy across forward and spot phases. In the spot market (C2 and FS2 treatments), the possible choices are zero, collusive, Cournot, defecting, and punishing output, which reflect pure strategies. In the FS2 treatment, the quantity choices are calculated based on the residual demand (total demand less forward sales).

I provide subjects with a detailed payoff table that lists all possible outcomes. Subjects are knowledgable of their own and their competitors' profit in any feasible stage-game outcome. (A copy of the instructions can be found in Appendix A.3.) Further, in all treatments, subjects can perfectly monitor the choices made by their competitor(s).

1.3.2 Demand Specification

The demand side is automated and subjects have zero production costs ($\gamma = 0$). The inverse demand is given by

$$p_{m,t} = \max\{120 - q_{m,t},\, 0\} \tag{1.3.1}$$

where $q_{m,t}$ denotes the total units sold in market m in round t. As stated above, I strictly impose forward-spot price parity in the FS2 treatment: $p^s_{m,t} = p^f_{m,t} = p_{m,t} = 120 - f_{m,t} - s_{m,t}$, where $f_{m,t}$ and $s_{m,t}$ respectively denote total units sold in the forward and spot phase. This assures that the conditions of the game in the experiment are as close to theory as possible without affecting the testable hypotheses. Importantly, subjects receive the same price for any units sold in either forward or spot phase. In each round, a subject's total profit is calculated as the product of their individual total production times the market price.

Table 1.2 lists the different strategy choices by treatment. In both duopoly treatments, there are five output choices in the spot phase stage-game. In the C3 treatment however, the defecting and punishing output quantities are equivalent, $q_j = 40$. Therefore, subjects could only choose from a set of four different quantities in the C3 treatment. In the FS2 treatment, subjects can play the collusive strategy in two different ways: either selling zero units forward and 30 units in the spot phase or selling 24 units forward and 6 units in the spot phase, respectively. This yields four different collusive subgame perfect equilibria in the FS2 treatment. Table 1.3 contrasts the collusive, Cournot, and defecting outcome predictions for all three treatments. Notice that selling forward makes the defecting strategy less tempting in the spot phase of the stage-game in the FS2 treatment.

Table 1.2: Sales Choices by Phase, by Treatment

	f_j	s_j
C2	-	$\{0, 30, 40, 45, 60\}$
FS2	$\{0, 24\}$	$\{0, (30 - f_j), (120 - f)/3, (90 - f_j)/2, (120 - f)/2\}$
C3	-	$\{0, 20, 30, 40\}$

Table 1.3: Collusive, Cournot, and Defecting Outcome Predictions by Treatment

		f_j	s_j	q_j	f	s	q	p	π_j	Efficiency
Collude	C2	-	30	30	-	60	60	60	1,800	75%
	FS2	{0, 24}	{30, 6}	30	{0, 24, 48}	{60, 36, 12}	60	60	1,800	75%
	C3	-	20	20	-	60	60	60	1,200	75%
Cournot	C2	-	40	40	-	80	80	40	1,600	89%
	FS2	24	24	48	48	48	96	24	1,152	96%
	C3	-	30	30	-	90	90	30	900	94%
Defect	C2	-	45	45	-	75	75	45	2,025	86%
	FS2	0	45	45	{0, 24}	{75, 51}	75	45	2,025	86%
	FS2	24	33	57	{24, 48}	{63, 39}	87	33	1,881	92%
	C3	-	40	40	-	80	80	40	1,600	89%

Note: The defectiving outcomes are calculated based on the assumption that the other firm(s) play the collusive strategy.

The implied critical discount factors in the experiment are $\underline{\delta} = 9/17$ in the C2 treatment, $\underline{\delta}(\lambda_j = 0.8) = 1/9$, $\delta(\lambda_j = 0) = 25/97$ in the FS2 treatment, and $\underline{\delta} = 4/7$ in the C3 treatment. The punishing strategy in the stage-game allows subjects to play a more severe grim strategy than just the Nash-reverting strategy. This implies lower critical discount factors of $\underline{\delta} = 1/9$ in the C2 treatment, $\underline{\delta}(\lambda_j = 0.8) =$

9/209, $\delta(\lambda_j = 0) = 1/9$ in the FS2 treatment, and $\underline{\delta} = 1/4$ in the C3 treatment.

1.3.3 Termination Rule

My design implements a repeated game with uncertain end, which, according to the Friedman theorem, allows for several subgame equilibria to exist (Friedman, 1971). Subjects interact with the same matched subject(s) for many rounds (fixed matching), but they do not know the exact number of rounds until the end of the experimental session. Normann and Wallace (2012) show that the termination rule in prisoner dilemma games does not significantly affect cooperation but may influence how subjects can maintain cooperation over time and its influence on end-of-game effects (see also Selten and Stoecker (1986)). Further, the authors find that the number of rounds significantly increases cooperation rates. The two-phase duopoly game is a complicated market mechanism; therefore, I refrain from using a stochastic termination rule with continuation probability to avoid unnecessary confusion of subjects' comprehension of the mechanism. Initially, I considered two different termination rules: known-end (subjects learn the exact number of rounds at the beginning of the session) and unknown-end. Specifically, I employed the known-end termination rule in one C2 and one C3 session. In comparing outcomes, I find no statistically significant difference between the unknown-end and known-end C3 sessions. In testing for end-of-game effects, I find that, on average, subjects chose higher outputs (more competitive strategies) in the final round of the known-end C2 session compared to the average output in 10 rounds prior. Therefore, I exclude the final round observations in the known-end C2 session from the analysis. Appendix A.1 shows the statistical analysis of the termination rules and end-of-game effects in detail.

1.3.4 Procedures

The data was collected in seven experimental sessions at the University of Tennessee, Knoxville in the Spring and Summer semesters of 2012. A total of 144 undergraduate student subjects participated in the sessions. Each subject participated in one session only. Each session consisted of at least 25 rounds[8] and lasted between one hour and one hour 30 minutes (the FS2 sessions lasted longer than the Cournot sessions due to the two-phase format). Subjects earned $23 on average.

At the beginning of each session, subjects were randomly and anonymously matched with one (two) other subject(s). Subjects were informed that they will interact with the same other subject(s) for several rounds. A monitor read the experimental instructions and explained the computer program to participants. The monitor thoroughly described the payoff table that accompanied the instructions. To verify that subjects understood how their earnings were calculated, the computer program asked each subject four practice questions before the start of the experiment. The computer program also displayed a payoff table in each decision round that listed all feasible sales combinations along with payoffs. In the second phase of the FS2 treatment, the computer program updated this payoff table conditional on the sales decisions in the first phase.

In each round, each participant had to choose an output amount from a list on the computer screen. After all participants submitted their sales decisions, the computer program determined the total units sold and price in each market. (At the end of the first phase in the FS2 treatment, subjects only observed the forward sales of their competitor and total forward sales in their market.) At the end of each round, each subject learned the total output of the other subject(s) in their market,

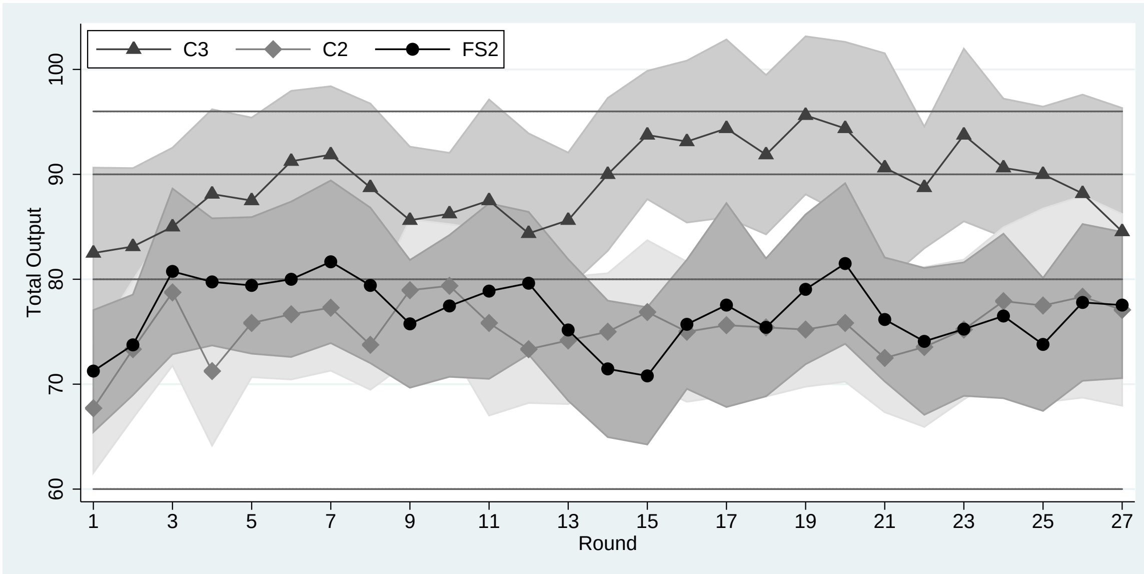
[8]The two known-end termination rule sessions consisted of 25 rounds each.

the total market output, the resulting market price, and their profit for that round. The computer program summarized and updated the market outcomes from previous rounds in the form of a table that was displayed on the computer screen at the time subjects submitted their decisions. (Appendix A.4 shows screen shots of the FS2 treatment.) All treatments were programmed in z-Tree (Fischbacher, 2007).

1.4 Experimental Results

1.4.1 Market Efficiency

First, I analyze the results in terms of total output and market efficiency. Figure 1.1 plots the average total output in each round by treatment along with 95% confidence bands. Horizontal lines at 60, 80, 90, and 96 denote respectively the collusive, standard duopoly stage-game equilibrium, three-firm stage-game equilibrium, and two-phase duopoly stage-game equilibrium output. The figure shows that the average two-phase duopoly output (black circles) is not different from the average standard duopoly output (light gray diamonds). Further, the average total output in the two-phase duopoly is far less than the predicted two-phase stage-game equilibrium quantity of 96 units. In both duopoly treatments, the average total output fluctuates at or below the standard stage-game equilibrium amount of 80 units. The aggregate three-firm output (gray triangles) oscillates around the stage-game equilibrium amount of 90 units. The graph also indicates that total output in both duopoly treatments is less than in the three-firm treatment. Figures A.1, A.2, and A.3 in Appendix A.2 show the total output by individual markets. These graphs indicate that outcomes are heterogeneous across markets. Some markets maintain either the collusive or the standard stage-game Cournot output for the majority of the rounds.



Note: Horizontal lines at 60, 80, 90, and 96 denote respectively the collusive, standard duopoly stage-game equilibrium, three-firm stage-game equilibrium, and two-phase duopoly stage-game equilibrium output.

Figure 1.1: Average Total Output per Round, All Treatments

In other markets, total output is characterized by high volatility.

Table 1.4 lists the average total output (by phase), prices, seller profits, and market efficiency by treatment. Average total output in the two-phase duopoly treatment is not significantly different from average total output in the standard duopoly treatment (Wilcoxon rank-sum test, $p = 0.59$). The total quantity in the three-firm treatment is signficantly larger on average than the average total output in either duopoly treatment (Wilcoxon rank sum tests, $p < 0.01$). In all three treatments, average total output is significantly greater than the collusive output (60 units). In the two-phase duopoly treatment, average forward sales (20.70 units) are significantly less than 48 units and spot sales are significantly greater than 48 units. Subjects sell significantly more units in the spot phase than in the forward phase (see Table 1.5 for detailed test statistics, paired t-tests yield the same results).

Table 1.4: Summary Statistics, Average Market Outcomes by Treatment

	f_j	s_j	q_j	f	s	q	p	π_j	Efficiency
C2	-	37.61	37.61	-	75.33	75.21	44.79	1,572.79	84.52%
		(5.59)	(5.59)		(14.94)	(10.75)	(10.75)	(204.72)	(6.52%)
FS2	10.35	28.08	38.43	20.70	56.17	76.87	43.13	1,522.62	85.20%
	(7.99)	(8.00)	(4.99)	(13.42)	(13.67)	(8.87)	(8.87)	(184.67)	(5.34%)
C3	-	29.75	29.75	-	89.22	89.24	30.76	845.43	91.98%
		(3.75)	(3.75)		(14.51)	(8.42)	(8.42)	(175.75)	(3.92%)

Note: Standard deviations in parentheses. Each market (supergame) counts as a single observation to control for possible correlation within a market.

Table 1.5: Z-Statistics of One-Sample Wilcoxon Signed-Rank Tests (H_0: Row Variable = Column Value, H_a: Row Variable $\neq$ Column Value) and Two-Sample Wilcoxon Rank-Sum Tests (H_0: Row Variable = Column Variable, H_a: Row Variable $\neq$ Column Variable)

	48	60	80	90	96	q_{C2}	s_{FS2}	f_{FS2}	q_{FS2}	q_{C3}
q_{C2}	-	4.04***	-1.66*	-	-	-	-	-	-0.54	-3.87***
s_{FS2}	2.54**	-1.37	-	-	-	-	-	4.09***	-	-
f_{FS2}	-4.29***	-	-	-	-	-	-4.09***	-	-	-
q_{FS2}	-	4.14***	-1.16	-	-4.29***	0.54	-	-	-	-3.99***
q_{C3}	-	3.52***	-	-0.49	-	3.87***	-	-	3.99***	-

Note: p-values given beneath. Significance at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively. Each market (supergame) enters the tests as a single observation to control for possible correlation within a market.

Table 1.6 contrasts market efficiency to the findings of Le Coq and Orzen (2006).[9] Market efficiency in the standard duopoly treatments does not differ between the

[9] Le Coq and Orzen (2006) had four treatments: (1) standard duopoly, (2) two-phase duopoly, (3) standard four-firm oligopoly, and (4) two-phase four-firm oligopoly with an inverse demand function of the form $p = 1,000 - q$. The authors use a fixed-matching protocol with known-end termination rule (25 rounds).

studies (two-paired t-test, $p = 0.70$). However, market efficiency in the two-stage duopoly treatment of Le Coq and Orzen (2006) is significantly higher than in this study (two-paired t-test, $p < 0.01$). This comparison indicates that strict forward-spot price parity significantly affects market-efficiency. In the set-up of Le Coq and Orzen (2006), sellers could not play a cooperative strategy that involved forward sales.

Table 1.6: Market Efficiency by Treatment

	This Study		Le Coq and Orzen	
	C2	FS2	C2	FS2
Efficiency	84.52%	85.20%	83.73%	91.71%
	(6.52%)	(5.34%)	(5.74%)	(5.00%)
N	24	24	15	15

Note: Standard deviations in parentheses.

Observations are likely dependent upon each other within a group of matched subjects and across time. I account for these potential inter-dependencies using a linear regression with cluster-robust standard errors. The model tests whether total output and market efficiency differ across the three treatments. Table 1.7 shows the estimation results. Specification 2 allows for a cubic time trend. Specification 3 allows for the cubic time trend to differ between the duopoly treatments and for a quadratic time trend in the C3 treatment. In all three specifications, total output and efficiency in the three-firm treatment are significantly greater than in either two-firm treatment (t-test with significance at the 1% level). However, the coefficient estimate on C2 is not significantly different from zero. The coefficient estimates on the time trend terms in specification 3 indicate that both output and efficiency exhibit oscillatory patterns in the duopoly treatments. Efficiency in the three-firm treatment is increasing at a decreasing rate over time.

Table 1.7: Effect of Treatment on Total Output and Efficiency

	Output			Efficiency (in %)		
	(1)	(2)	(3)	(1)	(2)	(3)
Constant	76.87***	72.11***	73.47***	85.20***	82.49***	83.77***
	(1.79)	(2.38)	(3.13)	(1.08)	(1.54)	(2.01)
C2	-1.54	-1.50	-4.74	-0.61	-0.58	-3.62
	(2.80)	(2.80)	(4.34)	(1.69)	(1.69)	(2.98)
C3	12.35***	12.37***	8.73**	6.76***	6.77***	5.18**
	(2.75)	(2.75)	(4.10)	(1.45)	(1.45)	(2.52)
Round	-	1.30**	-	-	0.82***	-
		(0.53)			(0.31)	
Round^2	-	-0.09**	-	-	-0.06**	-
		(0.04)			(0.02)	
Round^3	-	2.0E-03*	-	-	0.0014**	-
		(1.1E-03)			(0.0006)	
C2·Round	-	-	2.09*	-	-	1.39***
			(0.77)			(0.52)
C2·Round^2	-	-	-0.17**	-	-	-0.11***
			(0.06)			(0.04)
C2·Round^3	-	-	0.004**	-	-	0.003**
			(0.002)			(0.001)
FS2·Round	-	-	1.54	-	-	0.77
			(0.95)			(0.53)
FS2·Round^2	-	-	-0.14*	-	-	-0.07
			(0.08)			(0.04)
FS2·Round^3	-	-	0.003*	-	-	0.002*
			(0.002)			(0.001)
C3·Round	-	-	1.02***	-	-	0.44*
			(0.38)			(0.21)
C3·Round^2	-	-	-0.03**	-	-	-0.012*
			(0.01)			(0.006)
F	13.65	7.26	4.36	15.11	7.45	4.30
R^2	0.12	0.13	0.13	0.11	0.12	0.12

Note: $N = 1,682$ (64 markets (supergames) with 24 (25) to 27 observations per market). FS2 is the control group. Cluster-robust standard errors in parantheses. Significance of coefficient estimates at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

These findings indicate that there are no significant differences in total output and efficiency between the two duopoly treatments. I conclude that

Result 1.1. Market efficiency (output) in two-phase duopolies is not different from market efficiency (output) in single phase duopolies.

This result rejects hypotheses 1.1 and 1.2. Further, I reject hypothesis 1.3. The following two findings indicate why market efficiency does not differ across the two duopoly treatments. First, on average, neither firm committed to any forward sales in 38% of individual two-phase duopoly stage-games which means that sellers faced the single phase Cournot stage-game in more than one third of individual stage-games. In 20 out of 24 markets, both firms avoided forward sales in at least one round. Both firms sold in the forward phase in only 24% of all individual market outcomes. This shows that the forward market does not necessarily create a Prisoner's dilemma as Allaz and Vila (1993) predict and previous experiments suggest (Le Coq and Orzen, 2006; Brandts et al., 2008). Second, as outlined in section 1.2, firms can sustain collusive equilibria in the two-phase duopoly game even when they have short forward positions. The following discussion examines the latter conjecture by analyzing strategy choices in the spot phase of the stage-game. Recall that the experimental design supports collusive equilibria with short forward positions, whereas previous designs did not.

1.4.2 Strategy Choices

Figure 1.2 contrasts the distributions of chosen (stage-game) strategies in the duopoly treatments. Standard normality tests suggest that both distributions have a positive skew (Skewness/Kurtosis tests $p > \chi^2 < 0.01$, Shapiro-Wilk Normality tests, $p >$

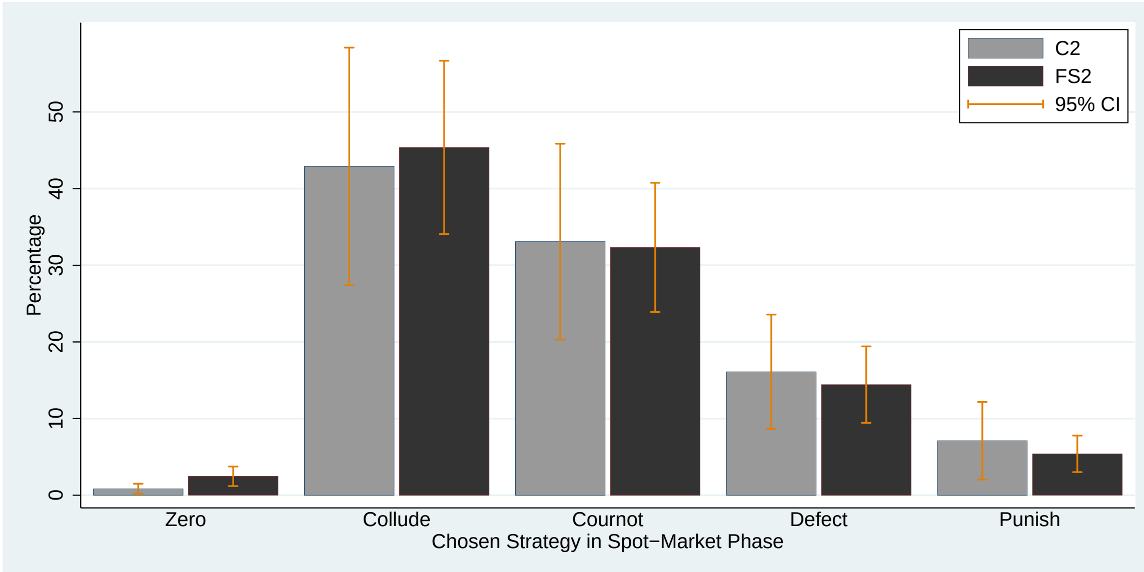


Figure 1.2: Percentage of Strategies by Two-Firm Treatment

$z < 0.01$). Sellers chose the collusive and Cournot (stage-game) strategies most frequently in both treatments. Whereas the difference between the collusive and Cournot strategies is not significant in either treatment (two-sample Wilcoxon signed rank test, $z_{FS2} = 1.23$, $p_{FS2} = 0.22$, $z_{C2} = 0.74$, $p_{C2} = 0.46$), all other differences between strategies are significant at the 1% level within each treatment. In both two-firm treatments, comparing the frequency of chosen strategies results in the following order: Collude, Cournot > Defect >Punish > Zero.

The chart in Figure 1.2 also shows that sellers chose the collusive strategy more frequently in the FS2 treatment than in the C2 treatment. Further, sellers chose the defective and competitive strategies less frequently in the FS2 treatment than in the C2 treatment. However, these differences are not significant (all strategies jointly: Kruskal-Wallis test, $\chi^2 = 0.92$, $p = 0.34$; individual strategies: test of proportions with p-values ranging from 0.66 to 0.96). Note that decisions in the experimental markets are very heterogeneous. (Figures A.4, A.5, and A.6 in Appendix A.2 show the distribution of chosen strategies by market.)

As a robustness check, I jointly test whether there are differences in distribution of chosen (stage-game) strategies in a multinomial logit model with standard errors clustered at the market level. Table 1.8 reports the estimation results. The coefficient estimate on the C2 indicator variable is not significantly different from zero for all strategies, which confirms that there are no significant differences in distribution between the C2 and FS2 treatment. An interesting result is that sellers chose the defective strategy less often in later rounds relative to the collusive strategy. Also, in both treatments, sellers chose the zero output strategy (dominated strategy) less often in later rounds.

Table 1.8: Effect of Type of Two-Firm Treatment on Strategy

	Zero		Cournot		Defect		Punish	
	Coefficient	Marginal Effect	Coefficient	Marginal Effect	Coefficient	Marginal Effect	Coefficient	Marginal Effect
Constant	-1.93***		-0.31		-0.79***		-2.13***	
	(0.33)		(0.28)		(0.29)		(0.37)	
C2	-1.09**	-1.48%	0.09	0.93%	0.17	1.64%	0.34	1.72%
	(0.53)	(0.63%)	(0.41)	(7.39%)	(0.44)	(4.41%)	(0.52)	(2.75%)
Round	-0.081***	-0.09%	-0.002	0.12%	-0.026*	-0.32%	-0.00003	0.04%
	(0.022)	(0.03%)	(0.012)	(0.25%)	(0.015)	(0.15%)	(0.021)	(0.12%)

Log-Likelihood $= -3,143.62$; Wald $\chi^2 = 35.82$; $N = 2,520$ (48 markets)

Note: FS2 is the control group. Base strategy is collude. The multinomial logit model estimates a set of coefficients for each strategy other than the base strategy. Coefficient estimates for different strategies are shown across columns. Cluster-robust standard errors in parantheses. Significance of coefficient estimates at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.

Figure 1.3 shows the distribution of strategies in the C3 treatment. Sellers chose the Cournot strategy most frequently. However, the difference between collusive and

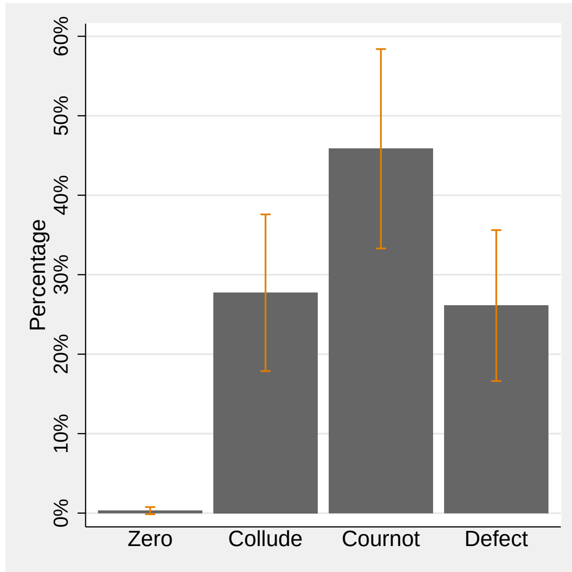


Figure 1.3: Percentage of Strategies in C3 Treatment

Cournot strategies is not significant (Wilcoxon signed rank test, $z = -1.58$, $p = 0.11$). Although subjects did not choose the collusive strategy significantly more often than the defective strategy ($z = 0.13$, $p = 0.90$), they chose the Cournot strategy significantly more often than the defective strategy ($z = 1.97$, $p = 0.05$). I do not test for differences in strategy distribution between the C3 and the duopoly treatments as the choice set in the C3 treatment consists of four choices only.

Next, I focus on the two-phase duopoly treatment only. To understand how forward market outcomes affect the output decisions in the spot phase of the stage-game, I analyze chosen spot-phase strategies conditional on the forward-phase outcome. Figure 1.4 reports the percentages of subjects' chosen strategies in the spot market by forward market outcome. A given firm (subject) can observe the following four forward-phase outcomes: (1) both the other firm and I sold 24 units each, (2) only the other firm sold 24 units, (3) only I sold 24 units, and (4) both the other firm and

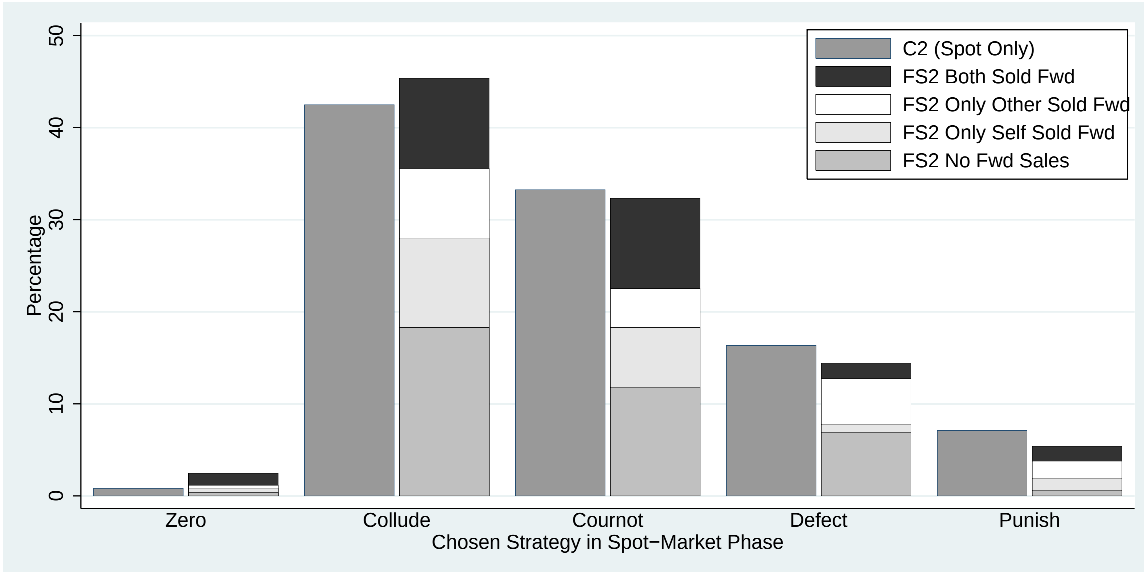


Figure 1.4: Percentage of Strategies by Two-Firm Treatment Conditional on Forward-Market Outcome

I refrained from selling forward. The graph indicates two important findings. First, if both firms sold forward they did not chose the Cournot strategy more often than if neither of them sold forward. Second, if a firm sold forward, it chose to defect less often. I test for differences in chosen strategies in the spot phase of the stage-game conditional on the outcome in the forward phase using a multinomial logit model.

Table 1.9 reports the estimation results that allows for a linear time trend. The binary variables 'Self Sold Forward', 'Other Sold Forward' and 'Self·Other' uniquely describe the four possible forward market outcomes. There are no observable differences between sellers choosing either the collusive or the Cournot strategy conditional on the forward market outcome. However importantly, subjects were less likely to choose the defective strategy if they sold in the forward market phase. Based on the marginal effect, firms that hold forward positions are 15.7% less likely to defect in the spot market relative to the collusive strategy. This finding indicates that a firm can commit to the collusive strategy more decidedly by selling forward. The following two results summarize the findings of the two-phase duopoly treatment:

Result 1.2. In duopolies with a single forward market opening, the collusive outcome can be sustained across both phases.

Result 1.3. A single forward market opening can soften competition in duopoly markets.

Result 1.2 supports hypotheses 1.4 and 1.5. Overall, the experimental results strongly suggest that forward markets may not have a pro-competitive effect. On the contrary, a forward market can make it easier for firms to maintain the collusive outcome.

Allowing subjects to play the punishing strategy (i.e. firms in a market produce an equal share of the maximum demand) leads to behavioral phenomena such as negative reciprocity. Some subjects play a collusive strategy in early rounds. Their competitors, however, play the defective strategy repeatedly early in the supergame[10]. Subjects then reciprocate by choosing the punishing output in later rounds. The data reveals these patterns in several supergames in both duopoly treatments. This behavior indicates that the punishing strategy is a viable grim strategy. The main results are unaffected by this behavioral effect.

[10]A supergame refers to several consecutive rounds of the same stage-game in a group of matched sellers

Table 1.9: Effect of Forward Market Outcome on Spot Market Strategy Choice

	Zero		Cournot		Defect		Punish	
	Coefficient	Marginal Effect	Coefficient	Marginal Effect	Coefficient	Marginal Effect	Coefficient	Marginal Effect
Constant	-3.00***		-0.33		-0.56		-3.20***	
	(0.55)		(0.46)		(0.44)		(0.60)	
Self Sold	0.75	1.62%	0.02	3.21%	-1.40**	-15.70%	1.37**	7.46%
Forward	(0.76)	(1.37%)	(0.57)	(10.13%)	(0.57)	(4.56%)	(0.67)	(3.57%)
Other	0.62	0.90%	-0.15	-9.35%	0.53	4.87%	1.97***	10.29%
Sold Forward	(0.70)	(1.12%)	(0.54)	(9.50%)	(0.52)	(3.83%)	(0.56)	(2.63%)
Self·Other	0.52	0.68%	0.56	14.04%	0.11	-0.79%	-1.75**	-5.98%
	(0.93)	(1.57%)	(0.81)	(15.27%)	(0.82)	(5.77%)	(0.86)	(1.70%)
Round	-0.068**	-0.11%	-0.007	0.03%	-0.031	-0.28%	-0.013	-0.02%
	(0.028)	(0.04%)	(0.016)	(0.33%)	(0.020)	(0.19%)	(0.030)	(0.12%)
Log-Likelihood $= -1,555.16$; Wald $\chi^2 = 193.13$; $N = 1,296$ (24 markets)								

Note: Control group is no forward sales. Base strategy is collude. The multinomial logit model estimates a set of coefficients for each strategy other than the base strategy. Coefficient estimates for different strategies are shown across columns. Cluster-robust standard errors in parantheses. Significance of coefficient estimates at the 1% and 5% level is denoted by ***, **, and *, respectively.

1.5 Discussion and Conclusion

In this article, I studied the strategic effect of forward sales on market efficiency and firms' output choices in infinitely repeated experimental duopoly markets. Although there is considerable heterogeneity in market outcomes within each treatment, I obtained the following robust results. First, a forward market does not act like additional competitors in an infinitely repeated setting. This findings is in contrast with previous experimental work on forward markets. Second, several collusive equilibria can be maintained in the presence of forward markets. Although I did not discover any differences in market efficiency between duopoly markets with and without forward sales, I found evidence that forward sales commitments can strengthen collusion as the defective strategy becomes less profitable in the spot-market. In my experiment, the collusive effect outweighed the increased quantity competition effect.

The experimental design in this article differs from previous experimental studies that test the strategic motive of forward contracts. One key design feature is that subjects can play several cooperative subgame perfect strategies that involve forward sales. Further, I impose strict forward-spot price parity to eliminate possible risk hedging motives. I achieve forward-spot price parity by restricting subjects' quantity choices to a discrete choice set. These design features support four collusive equilibria in the forward market duopoly treatment that were not supported in previous experiments.

The objective of this study was to design an environment that gives both the collusive predictions of Liski and Montero (2006) and the pro-competitive predictions of Allaz and Vila (1993) an equal chance. Nonetheless, the experiment abstracts from one important characteristic of forward markets: uncertainty. Both buyers and sellers can engage in forward contracts to hedge risk in future spot markets. Existing experimental work suggests that uncertainty and noise can yield cooperative outcomes in repeated Prisoner's dilemma games (Rojas, 2012; Fudenberg et al., 2012). Therefore, forward markets may increase the likelihood of collusive outcomes in the presence of uncertainty. Given the experiment results, important subsequent inquiries are whether and to what extent risk and uncertainty interact with the observed strategic effects.

The results of this study can assist antitrust authorities in mitigating market power in oligopolies that are characterized by few firms that interact repeatedly. A good example is the wholesale electricity industry: few sellers, homogeneous products that cannot be stored economically at a large scale[11], and sound forward markets. This article confirms that merely requiring electricity generators to sell forward, with the intent to limit their market power, can have the opposite effect as forward sales can strengthen collusive outcomes. Without strict regulation, two ways to mitigate market power in oligopolies are incentivizing entry and introducing forward markets. The findings provide evidence that incentivizing entry can be a superior market mechanism to forward markets.

[11]Two different spot markets are therefore independent markets and standard storage-based arbitrage arguments do not apply.

Chapter 2

Environmental Quality and Investor Behavior: Stock Market Reactions to Ecological Disasters

2.1 Introduction

Ecological disasters, like the extensive damage to marine and wildlife habitats following the explosion of the *Deepwater Horizon* oilrig in 2010, initiate lively and widespread debates about safety and sustainability concerns in relation to resource extraction and energy production. The intensive media coverage, in turn, leads to increased public awareness of the importance of environmental stewardship. Following an ecological disaster, consumers and investors update their beliefs not only about the vulnerability of ambient environmental quality but also about the impacts of corporate actions on the environment (in the case of man-made ecological disasters).

Stock market movements can provide information about the consequences of an ecological disaster. Stock market movements may also indicate whether and to what extent investors attribute importance to the cause of pollution and the associated loss in environmental quality. In this article, I investigate whether ecological disasters yield market-wide spillover effects. I analyze the stock market response to three ecological disasters: the Iceland Volcano eruptions in 2010, the BP oil-spill in 2010, and Hurricane Irene in 2011. All three disasters were well publicized in the media, however, they differed in their cause and their impact on envrionmental quality. The volcano eruptions were the results of seismic activity in Earth's interior. The eruptions led to the emission of carbon-dioxide and other greenhouse gases. The BP oil-spill was a man-made disaster that caused widespread damages of beaches and marine habitats. Hurricane Irene was a tropical cyclone that had neglible impacts on environmental quality. Nonetheless, some argue that natural ecological disasters like Irene are likely the result of global warming and as such society as a whole is responsible.[1] Hence, natural ecological disasters can heighten climate change awareness - there is empirical evidence that there is a relationship between climate change awareness and investor behavior (Jacobsen, 2011). However, Bulte et al. (2005) find evidence that willingess to pay to offset offset naturally caused environmental damages is significantly lower than willingness to pay to offset man-made environmental damages. I chose these three distinct ecological events to shed some light on these possible cause-related responses.

I conduct market-wide event studies that look at stock market reactions in industries that are not necessarily closely related to the disaster. Specifically, I examine abnormal returns of the 500 biggest publicly listed companies in the U.S. (by market

[1] Following Hurricane Sandy in 2012, Bloomberg Businessweek ran a cover story titled "It's Global Warming, Stupid" which argued that there is strong link between global warming and the frequency and severity of ecological disasters (Barrett, 2012).

value). While some companies may be more closely associated to a disaster than others, the majority of the companies are in unrelated industries. I examine whether firms with lower environmental impacts ("green" companies) experienced relatively higher abnormal returns than firms with higher environmental impacts ("brown" companies). My empirical approach differs from previous event studies in that I identify the extent to which investors care about companies' environmental stewardship by exploiting variation in firms' environmental performance. My empirical findings provide some evidence that investors reward positive environmental performance. However, this finding is not generalizable to all industries.

Previous research found that investors react to news about environmental pollution that is caused by publicly traded firms. These reactions translate into negative abnormal returns for the polluting firms' stock following publication of pollution news (Hamilton, 1995). There is also evidence that these negative abnormal returns are strongly correlated to the amount of media coverage and the magnitude of pollution (Hamilton, 1995; Capelle-Blancard and Laguna, 2010). Companies that violate environmental laws and thus cause environmental pollution are subject to severe financial obligations from legal and regulatory actions that translate into significant market value losses (Karpoff et al., 2005). Stock market reactions in response to man-made ecological disasters could theoretically be attributed to legal and reputational penalties. However, Karpoff et al. (2005) find that polluting firms' market value losses result mainly from legal and regulatory penalties rather than reputational penalties.

Research on stock market reactions to violations of environmental regulation and environmentally harm-inducing industrial accidents typically focuses only on the polluting firms. Nonetheless, empirical work suggests that negative events send market-wide signals about industry-specific risk and not just firm-specific risk (Knittel and

Stango, 2010). If an ecological disaster serves as a signal about industry-specific risk, one would expect stock value changes within the pollution-causing industry. Following the explosion of *Deepwater Horizon*, BP, plc stocks experienced negative cumulative abnormal returns of about 3% during the week following the announcement of the accident in the Wall Street Journal (Sabet et al., 2012)[2]. BP's subcontractors experienced negative cumulative abnormal returns ranging form 2% to 4% (Sabet et al., 2012). The oil spill also adversely affected other companies with offshore drilling operations in the US (Heflin et al., 2011). Importantly, Heflin et al. (2011) also provide evidence that market value losses were less severe for companies with offshore drilling operations that had detailed environmental disclosures. There is also empirical evidence that ecological disasters can affect shareholder wealth outside the core industry. For example, the Fukushima Daiichi nuclear disaster in March 2011 affected the market value of companies in the nuclear energy industry and also in the energy industry as a whole (Betzer et al., 2011; Ferstl et al., 2011; Lei et al., 2011; Lopatta and Kaspereit, 2011; Mama and Bassen, 2013). Nonetheless, one important question has not been answered: Do ecological disasters affect the market values of unrelated firms and industries? If so, do these spillover effects differ in the environmental performance of a company?

If investors care about the loss in environmental quality that is associated with an ecological disaster, then they may reward firms that have a positive environmental footprint and punish firms that have a poor environmental footprint even in unrelated (i.e. non-disaster) industries. There is empirical evidence that the diclosure of environmental performance information significantly impacts companies' shareholder

[2]The market value of BP, plc experienced an even greater drop of about 6% after the company was publicly held responsible for future cleanup costs (on April 28) under the Oil Pollution Act of 1990 (BOEMRE, 2011).

values (Lyon and Shimshack, 2012). Also, empirical work shows that consumers are willing to pay more for goods that are linked to a cause (e.g. Elfenbein and Mcmanus, 2010). In particular, there is evidence that consumers are willing to pay premiums for products that have lower environmental impacts (e.g. Casadesus-Masanell et al., 2009). Research on environmentally friendly consumption of impure public goods establishes a link between "green" preferences and the provision of environmental quality (Kotchen, 2005, 2006). Consumers and investors may feel responsible for environmental damages following an ecological disaster and may want to compensate for the losses through environmentally conscious purchasing and investment decisions (Kotchen, 2009). In this article, I empirically test whether "green" preferences translate into abnormal returns in the stock market following ecological disasters.

The article proceeds as follows. In section 2.2, I discuss the conceptual background. Section 2.3 describes the data and lays out the empirical specification. In section 2.4, I present the results from my empirical analysis. I discuss the findings in section 2.5.

2.2 Conceptual Framework

There are several reasons why an ecological disaster can yield spillover effects into unrelated industries. First, an ecological disaster likely affects investors' expectations about future environmental regulation which affects expected future dividends. Second, an ecological disaster likely leads to changes in the demand for products and services that companies offer, which also affects expected future dividends. If consumers care about the environment, the demand for products with lower adverse environmental impacts (during production or in use) likely shifts up, while the demand for products with higher adverse environmental impacts likely shifts down. Third, since an ecological disaster also raises awareness about the vulnerability of ambient environmental quality, investments in companies with higher environmental performance may increase for altruistic reasons. Heinkel et al. (2001) develop a simple two-firm equilibrium model of "exclusionary ethical investing" with a "green" technology and a "brown" technology firm. The authors postulate that if a large enough fraction of "green" investors refrains from investing in the "brown" company, then the "brown" stock price will fall as a result of decreased risk sharing among investors.[3]

Assuming that stock markets are in equilibrium, an ecological disaster is an exogenous shock that can result in observable stock market reactions in the short run. Potential stock price changes likely result from investors' changed expectations about future dividends that companies pay. Companies with lower environmental impacts are more likely to meet tighter environmental regulation than companies with higher environmental impacts. This yields an asymmetry in expected future dividends which likely translates into positive abnormal returns for "green" companies and negative abnormal returns for "brown" companies in the short run. Further, if investors believe that there is a large enough share of "green" consumers in the economy that will demand more "green" products and less "brown" products, they likely shift investments from "brown" companies to relatively more "green" companies. Additionally, if investors not only care about future dividends but also about the loss in ambient environmental quality due to the ecological disaster (e.g. loss of wildlife habitats), they may eschew "brown" company stocks and invest more in "green" company stocks. However, it remains an empirical question how investors respond to ecological disasters. I hypothesize that ecological disasters yield cumulative abnormal returns in

[3]Hong and Kacperczyk (2009) provide empirical support for this "ethical investment" hypothesis; they show that norm constrained investors abstain from alcohol, tobacco, and gaming stocks which translates into relatively higher expected returns for these stocks.

unrelated industries and that these abnormal returns differ based on a company's environmental performance. Specifically, "green" companies will experience relatively higher returns than "brown" companies.

Empirically, it is difficult to dissentangle the effects of investors' changed expectations about future dividends and responsibility effects due to losses in environmental quality. I explore these different channels by looking at disasters which differ in their characteristics. A man-made ecological disaster likely results in tighter environmental regulation than a natural ecological disaster. In addition, consumer demand responses are likely stronger following a man-made disaster as willingess to pay to offset man-made environmental damages likely exceeds willingness to pay to offset naturally caused environmental damages (Bulte et al., 2005). Thus, it is more likely to observe abnormal returns following a man-made ecological disaster than following a natural disaster. Likewise, a natural disaster that causes environmental damages more likely leads to observable abnormal stock returns than a natural disaster that causes mainly material damages. However, if investors believe that the frequency and severity of natural disasters is a consequence of human-caused climate change then a natural disaster may translate into observable abnormal returns even if its environmental damages are few. Therefore, I investigate different discrete events. I conduct market event studies following the volcano eruptions in Iceland in 2010, the BP oil-spill in 2010, and Hurricane Irene in 2011. I expect the BP oil-spill to have the largest observable spillover effects. Further, I hypothesize that the volcano eruptions yield observable abnormal returns that can be explained by differences in companies' environmental performance. However, I do not expect to observe abnormal returns following Hurricane Irene that can be explained by the environmental performance score.

2.3 Data and Empirical Method

In the empirical analysis, I use firm-specific financial data on the 500 biggest US companies (by market value for the fiscal years 2009 and 2010). I obtained daily financial market data from the Center for Research in Security Prices (CRSP) which I matched with annual firm specific accounting data from CompuStat. Further, I obtained data on these companies' environmental performance from Newsweek's Green Rankings publication. Since 2009, Newsweek annually publishes detailed green rankings of the 500 largest US publicly listed companies. In collaboration with KLD Research and Analytics, Trucost, and CorporateRegister.com, Newsweek assessed firms' environmental disclosure, impact and management, which then entered a "green score". The green score is the statistical average of three components: (i) the environmental impact score, (ii) the green policies score, and (iii) the reputation score. I take advantage of variation in companies' environmental impact score, which is a quantitative performance measure that reflects a company's environmental footprint under a "polluter pays" system as a percentage of its annual revenue (Newsweek, 2009). Trucost uses publicly disclosed environmental data such as carbon and other greenhouse gas emissions, water use, solid waste disposal, metals and chemicals as scores in the environmental impact score. To avoid confusion, hereafter, I refer to this measure as a company's environmental performance score. On a 100-point scale, the highest possible score is 100 (most "green") and the lowest possible score is 0 (least "green"). The environmental performance for the companies included in the study is approximately uniformly distributed over the interval 0 to 100 with a mean of 50.32 and standard deviation of 28.83. Tables 2.1 and 2.2 present summary statistics for the 2009 and 2010 sample of included companies by industry sector. I follow the industry specification as published by Newsweek in their green rankings. The banks and insurance and the financial services industries are the industry sectors with the highest environmental performances. The food and beverage and the utilities industries are the sectors with the lowest environmental performance scores in the sample.

Table 2.1: Industry-Specific Summary Statistics, Fiscal Year 2009

Industry	Firms	Mean Env. Performance	Mean Mkt. Value† (billion US$)	Mean Sales‡ (billion US$)	Mean EpS⋆ (US$)
All	492	50.12%	20.65	17.63	2.00
Banks and Insurance	35	93.50%	26.91	27.44	1.02
Basic Materials	28	16.35%	11.44	8.56	1.24
Consumer Products, Cars	29	37.90%	15.17	14.47	1.62
Financial Services	29	84.50%	19.61	7.98	2.94
Food & Beverage	28	11.00%	22.39	15.47	2.69
General Industrials	27	32.93%	13.88	13.98	1.67
Health Care	27	67.13%	15.06	17.16	3.39
Industrial Goods	47	52.85%	8.84	8.84	1.60
Media, Travel, Leisure	33	55.92%	14.79	12.33	0.94
Oil and Gas	31	30.96%	35.36	30.66	1.39
Pharmaceuticals	15	46.53%	52.77	17.35	2.74
Retail	52	63.76%	15.75	32.62	2.20
Technology	53	68.33%	38.11	19.34	1.42
Transportation, Aerospace	21	48.19%	21.71	21.17	4.07
Utilities	37	11.68%	10.61	9.41	2.88

Notes: The 2009 fiscal year sample consists of 492 companies. The following eight firms (tickers) had either missing accounting data or could not be matched to the market return data from CRSP: BKC, FAF, FO, GENZ, KFT, LSTZA, SGP, and WYE.
†Market value equals to annual closing price (CompuStat variable 1,003) times common shares outstanding (CompuStat variable 183).
‡Sales: CompuStat variable 749.
⋆Earnings per Share: CompuStat variable 294.

To study the stock market effects of ecological disasters, I conduct a market event study (see Fama et al., 1969 and MacKinlay, 1997). For each company, I calculate the daily average abnormal return and cumulative average abnormal return following the ecological disaster. The abnormal return for a stock is the prediction error between observed return and predicted returns based on the performance of the market as a whole. Then, I explore the relationship between a company's environmental performance score and cumulative abnormal returns.

Table 2.2: Industry-Specific Summary Statistics, Fiscal Year 2010

Industry	Firms	Mean Env. Performance	Mean Mkt. Value† (billion US$)	Mean Sales‡ (billion US$)	Mean EpS⋆ (US$)
All	489	50.46%	23.77	19.75	2.92
Banks and Insurance	39	79.26%	31.11	28.13	3.02
Basic Materials	27	16.90%	13.74	10.96	3.47
Consumer Products, Cars	30	43.99%	17.50	15.83	2.94
Financial Services	27	60.93%	23.12	9.85	4.04
Food & Beverage	26	10.25%	25.68	16.52	3.39
General Industrials	30	45.88%	15.39	13.57	2.29
Health Care	30	66.28%	13.69	17.75	3.32
Industrial Goods	40	56.84%	12.44	11.52	2.66
Media, Travel, Leisure	38	52.07%	17.61	12.92	2.21
Oil and Gas	28	32.55%	46.08	41.49	3.42
Pharmaceuticals	15	52.21%	50.77	20.45	2.54
Retail	56	60.33%	16.04	32.12	2.38
Technology	49	72.54%	45.61	2.32	2.71
Transportation, Aerospace	22	55.95%	22.39	20.96	4.47
Utilities	32	13.11%	12.12	10.55	2.31

Notes: The 2010 fiscal year sample consists of 489 companies. The following eleven firms (tickers) had either missing accounting data or could not be matched to the market return data from CRSP: BDK, BKC, CAL, EKDKQ, FO, GENZ, HEW.Z, KFT, LSTZA, and RGC.
†Market value equals annual closing price (CompuStat variable 1,003) times common shares outstanding (CompuStat variable 183).
‡Sales: CompuStat variable 749.
⋆Earnings per Share: CompuStat variable 294.

2.3.1 Event Study Methodology

At any given time t, the observed returns of a given security j, $R_{j,t}$, can be expressed as a function of the market return rate, $R_{m,t}$ (CRSP Value Weighted Index or S&P

500 Index)[4]:

$$R_{j,t} = \alpha_j + \beta_j R_{m,t} + \epsilon_{j,t}, \tag{2.3.1}$$

where α_j is a security specific average daily return independent of general market movements and $\epsilon_{j,t}$ captures any changes in security t's daily return that cannot be explained by either the security specific average daily return or by general market movements. The prediction error $\hat{e}_{j,t}$ is the difference between observed return at time t and estimated return (abnormal return):

$$\hat{e}_{j,t} = R_{j,t} - \hat{\alpha}_j - \hat{\beta}_j R_{m,t} = AR_j, \tag{2.3.2}$$

where $\hat{\alpha}_j$ and $\hat{\beta}_j$ denote the estimates of α_j and β_j, respectively.

Then, I calculate the cumulative abnormal returns (CAR) for each stock j for τ trading days following the event date t:

$$CAR_{j,\tau} = \sum_{\nu=t+1}^{t+\tau} AR_{j,\nu} \tag{2.3.3}$$

To determine the drivers of cumulative abnormal returns following an ecological disaster, I regress cumulative abnormal returns on the environmental performance score, x_{ej}, and I include industry fixed effects, $\boldsymbol{\vartheta_k}$. In my extended second specification, I allow for the effect of environmental performance to differ by industry:

$$CAR_{jk,\tau} = \boldsymbol{\vartheta_k} + \boldsymbol{\varphi\prime x_j} + \boldsymbol{\psi_k\prime x_{ej}} + \epsilon_{jk}, \tag{2.3.4}$$

where $\boldsymbol{x_j}$ is a vector of firm-specific control variables. The firm-specific covariates

[4] The notation follows MacKinlay (1997).

include the log linearized market value, log-linearized sales, and earnings per share (analogous to Lyon and Shimshack (2012) and Hong and Kacperczyk (2009)). The specification includes 15 industry fixed effects (vector $\boldsymbol{\vartheta_k}$) and 15 industry-specific coefficient estimates for the environmental impact score (vector $\boldsymbol{\psi_k}$).

2.4 Empirical Results

I report the empirical results of market event studies for three distinct ecological events: the carbon-dioxide emissions following the Iceland volcano eruptions, the BP oil-spill, and Hurricane Irene. The following market event studies are based on the CRSP value-weighted index as market portfolio. As a robustness check, I also conducted the analysis using the S&P 500 index as market portfolio, however, results do not differ significantly (see Tables B.1 through B.5 in Appendix B.1). I report the results based on a pre-event estimation period of 1 year (251 trading days). The results based on a pre-event estimation period of half a year do not vary much quantitatively or qualitatively (see section B.1.2 in the Appendix).

2.4.1 Eruptions of Eyjafjallajökull Volcano, 2010

In April 2010, volcanic events in Iceland at Eyjafjallajökull created a large ash cloud which covered large areas of northern Europe from April 14-20, 2010. The volcano emitted about 150,000 tons of carbon-dioxide per day (UNEP, 2011). The ash cloud severely disrupted air travel to and from Europe for several days. The volcano eruptions were a naturally occuring ecological disaster (seismic activity) that caused losses in ambient environmental quality (greenhouse gas emissions).

Table 2.3 presents the main regression results for an event window of six trading days following the eruptions (Wednesday, April 14 through Wednesday, April 21). The first column in each specification lists the coefficient estimates. The second column in specification 2 shows the estimated cumulative abnormal returns evaluated at the industry means, $\widehat{CAR}_k = \hat{\vartheta}_k + \hat{\boldsymbol{\varphi}}\boldsymbol{\prime}\overline{\boldsymbol{x}}_{\boldsymbol{k}} + \hat{\psi}_k \overline{x}_{ek}$, for each industry k. The coefficient estimate on environmental performance is not signifcant in the baseline regression. However, looking at average cumulative abnormal returns within a given industry, the results indicate that the volcano eruptions lead to positive cumulative abnormal returns in the following industries: consumer products and cars, general industrials, and transportation and aerospace. The average company in the basic materials, financial services, and pharmceuticals industries experienced significant negative cumulative abnormal returns. In the extended specification, the industry-specific environmental performance coefficients suggest that positive abnormal returns in the transportation industry were greater for companies with a higher environmental performance score. For example, cumulative abnormal returns of a transportation company were six basis points higher for each one unit increase in the environmental performance score. Similarly, companies in the basic materials and financial services sectors with higher environmental performance scores experienced relatively higher returns. Note that the coefficient estimate on environmental performance is significantly negative in the health care industry. Overall, some environmental performance scores are significantly postive and some are significantly negative. There is not enough convincing evidence that relatively more "green" companies fared better than "brown" companies in industries with significant abnormal returns.

Table 2.3: Iceland Volcano Estimates, 6-Day Event Window

	(1)		(2)			
Variable	CAR_{6d}		CAR_{6d}		Mean $\widehat{CAR}_{6d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	0.01	(0.01)	-			
Banks and Insurance	-		0.06*	(0.03)	0.99*	(0.51)
Basic Materials	-		0.11**	(0.04)	-2.75***	(0.75)
Consumer Products, Cars	-		-0.05*	(0.02)	1.91***	(0.49)
Financial Services	-		0.12**	(0.05)	-1.39**	(0.64)
Food & Beverage	-		-0.06	(0.07)	-0.56*	(0.34)
General Industrials	-		-0.03	(0.03)	2.16***	(0.65)
Health Care	-		-0.06**	(0.03)	-0.72	(0.50)
Industrial Goods	-		0.03**	(0.02)	0.56	(0.40)
Media, Travel, Leisure	-		0.02	(0.04)	-0.41	(0.80)
Oil and Gas	-		0.04	(0.07)	0.50	(0.48)
Pharmaceuticals	-		0.06	(0.07)	-3.48***	(0.78)
Retail	-		0.02	(0.05)	0.38	(0.43)
Technology	-		-0.03	(0.02)	0.47	(0.44)
Transportation, Aerospace	-		0.06**	(0.03)	1.92***	(0.61)
Utilities	-		-0.01	(0.02)	0.02	(0.33)
Firm-Level						
ln (Mkt. Value)	-0.12	(0.20)	-0.10	(0.20)	-	
ln (Sales)	0.01	(0.21)	0.06	(0.21)	-	
EpS	-0.09	(0.07)	-0.06	(0.06)	-	
N	484		484			
R^2	0.17		0.22			
F	4.46***		3.83***			

Notes: Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively. The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: ACS, BRK3, BDK, BKC, DTV, ETN, FAF, FO, GENZ, JAVA, KFT, LSTZA, PBG, PAS, SGP, WYE.

I chose an event window length of six days that spans the time when the ash cloud covered parts of Europe. The volcano eruptions were succeeded by another ecological disaster (*Deepwater Horizon* explosion), which precludes an analysis with a longer event window.

2.4.2 BP Oil Spill, 2010

The BP oil spill was a man-made ecological disaster which caused severe losses in ambient environmental quality (pollution of beaches, loss of marine wildlife and habitats, damage to ocean floor). I give a brief chronology of the key events associated with the oil spill. For a detailed timeline of events, see the joint report by the U.S. Coast Guard and the Bureau of Ocean Energy Management, Regulation and Enforcement (BOEMRE, 2011).

- On April 20, 2010, an explosion occurred on the deepwater oil drilling platform *Deepwater Horizon* which subsequently caused crude oil to gush freely into the Gulf of Mexico for several months.

- On April 22, the main operator, BP, plc (BP hereafter) reported the accident in the Wall Street Journal.

- On April 29, President Obama declared the disaster a Spill of National Significance and publicly held BP responsible for future cleanup costs, one day after the National Pollution Funds Center declared BP a responsible party under the Oil Pollution Act of 1990.

- On May 27, the U.S. government released a moratorium on deepwater oil drilling which went into effect on May 30.

- On Saturday, May 29, BP announced that the *top-kill* procedure failed to stop the flow of oil.
- On June 1, oil began washing up on the beaches of the Gulf Islands National Seashore , by June 6, the oil spill had landed on the coast of Louisiana, Mississippi, and Alabama.
- On June 16, BP established the Gulf Coast Claims Facility ($20 billion settlement fund).
- On July 15, the wellhead on the ocean floor was capped.

In computing abnormal returns, I used a one-year estimation period from April 22, 2009 to April 21, 2010.

2.4.2.1 Deepwater Horizon Oilrig Explosion

First, I study the effect of information disclosure about the accident. While BP made information about the accident available on Thursday, April 22, at the time, the extent of the oil spill and resulting environmental damages were not predictable. I chose event window lengths of 3, 6 and 11 days starting on April 23. The event window of length 6 (11) spans over one (two) weeks following the information disclosure. Table 2.4 reports the main regression results for an event window length of three days. The baseline model reports a significantly negative environmental performance score. However, this result is likely driven by positive abnormal returns in the oil and gas industry which has low environmental performance scores on average. Average cumulative abnormal returns are significantly positive in the basic materials, media, travel, leisure, and oil and gas industries. The average oil and gas company in the sample experienced positive cumulative abnormal returns of 215 basis points three days after the event. The average company in the banks and insurance, food and beverage, retail, and transportation and aerospace industries had significantly negative cumulative abnormal returns. The coefficient estimate on the environmental performance score is significantly positive in the retail industry in specification 2. For each one unit decrease in the performance score, cumulative abnormal returns decrease by eight basis points. However, the environmental performance coefficients are significantly negative in the financial services and transportation and aerospace sectors. Companies with relatively higher market value experienced higher negative abnormal returns (negative coefficient on log- market value).

Table 2.4: *Deepwater Horizon* Explosion Estimates, 3-Day Event Window

Variable	(1) CAR_{3d}		(2) CAR_{3d}		Mean $\widehat{CAR}_{3d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	-0.01**	(<0.01)	-			
Banks and Insurance	-		-0.04	(0.03)	-1.45***	(0.43)
Basic Materials	-		-0.05*	(0.03)	1.67***	(0.48)
Consumer Products, Cars	-		<0.01	(0.01)	0.72	(0.46)
Financial Services	-		-0.07**	(0.03)	0.01	(0.58)
Food & Beverage	-		0.05	(0.05)	-1.59***	(0.25)
General Industrials	-		0.01	(0.01)	0.68	(0.50)
Health Care	-		-0.03	(0.02)	-0.80*	(0.45)
Industrial Goods	-		-0.02*	(0.01)	0.61	(0.37)
Media, Travel, Leisure	-		0.02	(0.03)	1.50**	(0.68)
Oil and Gas	-		-0.04	(0.05)	2.15***	(0.36)
Pharmaceuticals	-		0.03	(0.03)	-0.15	(0.48)
Retail	-		0.08**	(0.04)	-0.63**	(0.31)
Technology	-		-0.03	(0.02)	-0.01	(0.27)
Transportation, Aerospace	-		-0.06**	(0.02)	-0.60**	(0.28)
Utilities	-		-0.02	(0.02)	0.15	(0.29)
Firm-Level						
ln (Mkt. Value)	-0.27*	(0.16)	-0.36**	(0.17)	-	
ln (Sales)	-0.01	(0.14)	0.08	(0.15)	-	
EpS	0.01	(0.05)	<0.01	(0.05)	-	
N	484		484			
R^2	0.18		0.21			
F	6.94***		5.32***			

Notes: Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively. The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: ACS, BRK3, BDK, BKC, DTV, ETN, FAF, FO, GENZ, JAVA, KFT, LSTZA, PBG, PAS, SGP, WYE.

I replicated the analysis using event window lengths of six and eleven days. Tables 2.5 and 2.6 report the results. Comparing the 3-day event window results to the 6-day results indicates that the observed effects did not persist one week following the event. Negative returns for the average company in the food and beverage and the retail industries were still significant yet smaller. However, the observed average abnormal returns in the banks and insurance, basic materials, and transportation and aerospace industries were no longer significant one week after the event. Average positive abnormal returns in the media, travel, leisure and oil and gas industry were still weakly significant at the 10% level, yet smaller in magnitude. Importantly, comparing the 3-day results to the 11-day results (Table 2.6) suggests that the event significantly affected several industries' abnormal returns. The average firm in the banks and insurance, basic materials, financial services, and the oil and gas industries experienced significant positive cumulative abnormal returns two weeks after the event. Average cumulative abnormal returns in the health care, retail, and technology sectors are negative. The industry-specific coefficient estimates on the environmental performance score are insignifcant for most industries. The environmental performance coefficient estimate is significantly negative for the financial services sector. However, companies in the financial services industry have high performance scores with little varation relative to the 100-point scale. The environmental performance coefficient estimate in the consumer products, cars and industrial sectors is significantly negative, although average companies in both sectors did not have significant cumulative abnormal returns. Note that cumulative abnormal returns are significantly negatively correlated with earnings per share two weeks after the event. The 11-day window includes the time period after president Obama publicly declared BP a responsible party under the Oil Pollution Act of 1990. I look the impact of the declaration of responsible parties in the next section.

Table 2.5: *Deepwater Horizon* Explosion Estimates, 6-Day Event Window

	(1)		(2)			
Variable	CAR_{6d}		CAR_{6d}		Mean $\widehat{CAR}_{6d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	-0.01	(0.01)	-			
Banks and Insurance	-		0.02	(0.04)	-0.09	(0.64)
Basic Materials	-		-0.03	(0.03)	1.20*	(0.62)
Consumer Products, Cars	-		-0.04**	(0.02)	0.91	(0.87)
Financial Services	-		-0.06	(0.06)	0.60	(0.72)
Food & Beverage	-		0.04	(0.09)	-0.94**	(0.43)
General Industrials	-		0.01	(0.04)	0.30	(0.94)
Health Care	-		-0.08*	(0.05)	-0.51	(0.62)
Industrial Goods	-		-0.05*	(0.02)	0.60	(0.64)
Media, Travel, Leisure	-		0.07*	(0.04)	1.53*	(0.90)
Oil and Gas	-		-0.07	(0.08)	1.86*	(0.97)
Pharmaceuticals	-		0.02	(0.06)	1.76**	(0.80)
Retail	-		0.09	(0.07)	-2.35***	(0.59)
Technology	-		$<$0.01	(0.04)	-1.50***	(0.54)
Transportation, Aerospace	-		-0.02	(0.03)	-0.39	(0.42)
Utilities	-		-0.03	(0.02)	1.38***	(0.52)
Firm-Level						
ln (Mkt. Value)	-0.14	(0.28)	-0.25	(0.30)	-	
ln (Sales)	0.06	(0.27)	0.17	(0.29)	-	
EpS	-0.07	(0.08)	-0.08	(0.08)	-	
N	483		483			
R^2	0.11		0.14			
F	3.09***		2.60***			

Notes: Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively. The following companies (tickers) were excluded from the regression either time series: ACS, BRK3, BDK, BJS, BKC, DTV, ETN, FAF, FO, GENZ, JAVA, KFT, LSTZA, PBG, PAS, SGP, WYE.

Table 2.6: *Deepwater Horizon* Explosion Estimates, 11-Day Event Window

Variable	(1) CAR_{11d}		(2) CAR_{11d}		Mean $\widehat{CAR}_{11d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	-0.03**	(0.01)	-			
Banks and Insurance	-		0.11*	(0.06)	4.75***	(0.84)
Basic Materials	-		-0.01	(0.04)	4.40***	(0.97)
Consumer Products, Cars	-		-0.07***	(0.02)	-0.34	(0.90)
Financial Services	-		-0.19***	(0.07)	3.51***	(1.11)
Food & Beverage	-		0.11	(0.14)	-1.30*	(0.69)
General Industrials	-		<0.01	(0.05)	-0.04	(1.40)
Health Care	-		-0.03	(0.06)	-1.98**	(0.85)
Industrial Goods	-		-0.08**	(0.04)	0.65	(0.72)
Media, Travel, Leisure	-		0.01	(0.05)	1.14	(1.05)
Oil and Gas	-		-0.06	(0.10)	2.96***	(0.91)
Pharmaceuticals	-		0.05	(0.06)	1.24	(0.80)
Retail	-		0.08	(0.08)	-2.68***	(0.69)
Technology	-		-0.01	(0.03)	-2.09***	(0.52)
Transportation, Aerospace	-		-0.06	(0.05)	-0.32	(0.69)
Utilities	-		-0.05	(0.04)	1.10	(0.68)
Firm-Level						
ln (Mkt. Value)	-0.24	(0.33	-0.30	(0.34)	-	
ln (Sales)	0.19	(0.31	0.28	(0.33)	-	
EpS	-0.22***	(0.07	-0.26***	(0.07)	-	
N	483		483			
R^2	0.22		0.25			
F	7.33***		5.10***			

Notes: Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.
The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: ACS, BRK3, BDK, BJS, BKC, DTV, ETN, FAF, FO, GENZ, JAVA, KFT, LSTZA, PBG, PAS, SGP, WYE.

2.4.2.2 Declaration of Responsible Parties

To isolate the effect of the declaration of responsible parties from the effect of the explosion, I conducted the main analysis using a 6-day event window that covers Friday, April 30 through Friday, May 7. Table 2.7 presents the regression results. The coefficient estimate on environmental performance is significantly negative in the basic specification. The average company in the following industries had significant positive returns six days after President Obama publicy declared BP a responsible party: banks and insurance, basic materials, financial services, oil and gas, and utilities. The average banks and insurance company had positive cumulative returns exceeding 600 basis points. The average company in the oil and gas industry experienced cumulative abnormal returns of 220 basis points. The average company in the health care, retail, and technology industries had significantly negative cumulative abnormal returns. Note that cumulative abnormal returns are significantly negatively correlated with earnings per share. The coefficient estimate on the environmental performance score is significantly positive for the banks and insurance industry, however, the average environmental performance score of companies in the banks and insurance sector was 93.50% with little variation relative to other companies in the full sample. The results do not provide evidence that differences in the environmental performance can explain the differences in cumulative abnormal returns.

Next, I analyze the stock market reaction to the failed *top-kill* procedure that coincided with the oil-spill making landfall on U.S. beaches in the Gulf of Mexico at the end of May (beginning of June).

Table 2.7: Oil-Spill Responsibility Disclosure Estimates, 6-Day Event Window

Variable	(1) CAR_{6d}		(2) CAR_{6d}		Mean $\widehat{CAR}_{6d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	-0.03**	(0.01)	-			
Banks and Insurance	-		0.15**	(0.06)	6.13***	(0.69)
Basic Materials	-		0.01	(0.04)	3.73***	(1.11)
Consumer Products, Cars	-		-0.04*	(0.02)	-1.09*	(0.62)
Financial Services	-		-0.11*	(0.06)	2.76***	(0.82)
Food & Beverage	-		-0.04	(0.13)	-0.27	(0.60)
General Industrials	-		-0.03	(0.04)	-0.72	(0.94)
Health Care	-		-0.03	(0.05)	-2.69***	(0.56)
Industrial Goods	-		-0.05*	(0.03)	-0.23	(0.63)
Media, Travel, Leisure	-		-0.04	(0.03)	-0.22	(0.62)
Oil and Gas	-		-0.03	(0.09)	2.20***	(0.69)
Pharmaceuticals	-		0.03	(0.04)	0.04	(0.75)
Retail	-		-0.03	(0.04)	-1.27***	(0.42)
Technology	-		0.02	(0.02)	-1.72***	(0.43)
Transportation, Aerospace	-		-0.03	(0.04)	-0.18	(0.49)
Utilities	-		-0.04	(0.03)	1.06**	(0.49)
Firm-Level						
ln (Mkt. Value)	0.13	(0.25)	0.15	(0.26)	-	
ln (Sales)	-0.01	(0.25)	0.01	(0.26)	-	
EpS	-0.28***	(0.07)	-0.31***	(0.07)	-	
N	483		483			
R^2	0.32		0.34			
F	10.02***		7.44***			

Notes: Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively. The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: ACS, BRK3, BDK, BJS, BKC, DTV, ETN, FAF, FO, GENZ, JAVA, KFT, LSTZA, PBG, PAS, SGP, WYE.

2.4.2.3 Oil-Spill Landfall on US Beaches

I chose event windows of four and nine days which cover one and two weeks after the initial landfall of the oil-spill. Three events coincide: the *top-kill* procedure failed, oil started washing up on U.S. beaches and the deepwater oil-drilling moratorium went into effect. Tables 2.8 and 2.9 show the regression results. The coefficient estimate on environmental performance is significantly positive at the 10% significance level which indicates that companies with higher environmental performance scores experienced relatively greater cumulative abnormal returns. Four days after the event, the average company in the basic materials, consumer products, cars, industrial goods, retail, transportation and aerospace, and utilities industries had estimated significant negative cumulative abnormal returns. The average company in the food and beverage industry had significantly positive cumulative abnormal returns. The coefficient estimates on environmental performance in the extended specification are significantly positive for the health care and retail industries. For each one unit increase in the performance score, abnormal returns were twelve basis points higher in the health care sector. However, the abnormal returns for the average company in the health care sector were not significant. In the retail industry, for each one unit increase in the impact score, cumulative abnormal returns were eight basis points higher. Companies with lower performance scores experienced relatively lower returns in these two industries.

This relationship persists and is stronger in absolute terms two weeks after the initial landfall. For each one unit increase in environmental performance, a company's estimated cumulative abnormal return increased by eleven (18) basis points in the health care (retail) industry. Two weeks after the event, cumulative abnormal returns were positively correlated with relative market value and negatively correlated with relative sales. In sum, the findings indicate that the BP disaster resulted in intra-industry and outside-industry spillover effects. Some of the variation in cumulative abnormal returns can be attributed to variation in environmental performance scores. However, this finding is not generalizable to all industries in the sample.

Table 2.8: Oil-Spill Landfall Estimates, 4-Day Event Window

	(1)		(2)			
Variable	CAR_{4d}		CAR_{4d}		Mean $\widehat{CAR}_{4d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	0.01*	(<0.01)	-			
Banks and Insurance	-		<0.01	(0.02)	0.03	(0.26)
Basic Materials	-		-0.02	(0.02)	-1.78***	(0.48)
Consumer Products, Cars	-		0.01	(0.02)	-1.19***	(0.50)
Financial Services	-		0.02	(0.03)	-0.06	(0.41)
Food & Beverage	-		-0.06	(0.08)	0.72**	(0.32)
General Industrials	-		-0.01	(0.02)	-0.15	(0.45)
Health Care	-		0.12***	(0.02)	0.17	(0.37)
Industrial Goods	-		-0.01	(0.01)	-0.88***	(0.31)
Media, Travel, Leisure	-		-0.01	(0.02)	0.49	(0.43)
Oil and Gas	-		0.21	(0.15)	1.08	(0.99)
Pharmaceuticals	-		0.05	(0.04)	-0.70	(0.57)
Retail	-		0.09***	(0.03)	-1.38***	(0.40)
Technology	-		0.03	(0.02)	0.49	(0.30)
Transportation, Aerospace	-		0.02	(0.02)	-1.56***	(0.26)
Utilities	-		-0.02	(0.01)	-0.69***	(0.21)
Firm-Level						
ln (Mkt. Value)	0.30	(0.18)	0.28	(0.17)	-	
ln (Sales)	-0.20	(0.19)	-0.18	(0.17)	-	
EpS	-0.01	(0.05)	-0.01	(0.05)	-	
N	483		483			
R^2	0.13		0.20			
F	5.22***		5.75***			

Notes: Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively. The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: ACS, BRK3, BDK, BJS, BKC, DTV, ETN, FAF, FO, GENZ, JAVA, KFT, LSTZA, PBG, PAS, SGP, WYE.

Table 2.9: Oil-Spill Landfall Estimates, 9-Day Event Window

Variable	(1) CAR_{9d}		(2) CAR_{9d}		Mean $\widehat{CAR}_{9d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	0.02	(0.01)	-			
Banks and Insurance	-		0.02	(0.04)	-0.80	(0.51)
Basic Materials	-		-0.05	(0.03)	0.04	(0.63)
Consumer Products, Cars	-		0.05*	(0.02)	-2.04**	(0.81)
Financial Services	-		0.05	(0.04)	-0.85	(0.56)
Food & Beverage	-		-0.16	(0.15)	1.95***	(0.56)
General Industrials	-		<0.01	(0.03)	-0.57	(0.77)
Health Care	-		0.11***	(0.03)	-0.67	(0.52)
Industrial Goods	-		-0.03	(0.02)	-1.14**	(0.50)
Media, Travel, Leisure	-		-0.02	(0.02)	-0.74	(0.67)
Oil and Gas	-		0.20	(0.15)	2.89**	(1.23)
Pharmaceuticals	-		-0.04	(0.08)	0.36	(0.83)
Retail	-		0.17***	(0.06)	-2.00***	(0.56)
Technology	-		0.04	(0.04)	-0.67	(0.56)
Transportation, Aerospace	-		0.03	(0.04)	-2.25***	(0.55)
Utilities	-		-0.02	(0.03)	1.12***	(0.38)
Firm-Level						
ln (Mkt. Value)	0.71***	(0.26)	0.65**	(0.26)	-	
ln (Sales)	-0.70***	(0.26)	-0.64**	(0.26)	-	
EpS	0.02	(0.06)	0.02	(0.06)	-	
N	483		483			
R^2	0.15		0.20			
F	4.23***		4.25***			

Notes: Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively. The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: ACS, BRK3, BDK, BJS, BKC, DTV, ETN, FAF, FO, GENZ, JAVA, KFT, LSTZA, PBG, PAS, SGP, WYE.

2.4.3 Hurricane Irene, 2011

From August 27-29 2011, Hurricane Irene's destructive path covered areas in North Carolina, Virginia, New Jersey and New York. Estimated damages around $15.6 billion (Avila and Cangialosi, 2012) make Irene the 7th costliest hurricane in U.S. history. Irene damaged homes and led to catastrophic inland flooding in New Jersey, Massachusetts and Vermont. Its environmental damages were mainly felled trees. Irene was a natural disaster that caused minor losses in ambient environmental quality. Tables 2.10 and 2.11 present the regression results for event window lengths of five and nine trading days. The environmental performance score coefficient estimates are not statistically significant in the basic specifications. Note that one week after the event, the average company in the basic materials, oil and gas, and utilities industries had significantly positive cumulative abnormal returns. The average company in the financial services and transportation industries had significantly negative returns. Transportation companies with lower environmental performance scores experienced relatively lower abnormal returns. Utilities companies with lower environmental performance scores experienced higher cumulative abnormal returns. However, variation in environmental performance cannot explain cumulative abnormal returns in any other sector.

Two weeks after the landfall, only average companies in the oil and gas, retail, and the technology industries had significantly positive cumulative abnormal returns. Note that neither the firm-level control variables nor environmental performance can explain the variation of cumulative abnormal returns within industries.[5]

[5] Utilities companies with lower environmental performance scores experienced higher cumulative abnormal returns. However, on average, utilities companies in the sample have low environmental impact scores with little variation relative to other industries.

Table 2.10: Hurricane Irene Estimates, 5-Day Event Window

Variable	(1) CAR_{5d}		(2) CAR_{5d}		Mean $\widehat{CAR}_{5d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	<0.01	(0.01)	-			
Banks and Insurance	-		0.02	(0.03)	0.22	(1.02)
Basic Materials	-		-0.01	(0.02)	0.68**	(0.32)
Consumer Products, Cars	-		-0.01	(0.02)	0.10	(0.42)
Financial Services	-		<0.01	(0.02)	-1.25**	(0.53)
Food & Beverage	-		0.09	(0.08)	0.46	(0.43)
General Industrials	-		-0.01	(0.02)	-0.23	(0.53)
Health Care	-		0.03	(0.02)	-0.05	(0.36)
Industrial Goods	-		-0.01	(0.02)	-0.14	(0.42)
Media, Travel, Leisure	-		-0.01	(0.02)	0.75	(0.46)
Oil and Gas	-		<0.01	(0.02)	1.26***	(0.38)
Pharmaceuticals	-		-0.01	(0.02)	0.76**	(0.32)
Retail	-		-0.04	(0.03)	-0.37	(0.46)
Technology	-		0.02	(0.02)	-0.34	(0.39)
Transportation, Aerospace	-		0.03***	(0.01)	-1.06***	(0.34)
Utilities	-		-0.04**	(0.01)	0.96***	(0.27)
Firm-Level						
ln (Mkt. Value)	-0.25	(0.19)	-0.28	(0.20)	-	
ln (Sales)	-0.05	(0.22)	-0.05	(0.23)	-	
EpS	-0.07	(0.04)	-0.07	(0.04)	-	
N	483		483			
R^2	0.07		0.08			
F	3.22***		2.88***			

Notes: Event window is August 29 - September 2. Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.
The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: BEC, BDK, BKC, CAL, DTV, EKDKQ, ETN, FO, GENZ, HEW.Z, KFT, LSTZA, MI., Q, RGC, SII, and THC.

Table 2.11: Hurricane Irene Estimates, 9-Day Event Window

Variable	(1) CAR_{9d}		(2) CAR_{9d}		Mean $\widehat{CAR}_{9d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	<0.01	(0.01)	-			
Banks and Insurance	-		0.01	(0.04)	0.84	(1.11)
Basic Materials	-		-0.02	(0.02)	0.73	(0.59)
Consumer Products, Cars	-		<0.01	(0.02)	-0.55	(0.53)
Financial Services	-		0.01	(0.02)	-0.59	(0.56)
Food & Beverage	-		0.02	(0.09)	-0.26	(0.49)
General Industrials	-		<0.01	(0.02)	0.02	(0.66)
Health Care	-		0.03	(0.03)	-0.95*	(0.52)
Industrial Goods	-		-0.01	(0.02)	0.08	(0.56)
Media, Travel, Leisure	-		-0.02	(0.03)	-0.18	(0.73)
Oil and Gas	-		<0.01	(0.03)	1.49**	(0.60)
Pharmaceuticals	-		<0.01	(0.03)	0.79	(0.72)
Retail	-		-0.03	(0.04)	1.08**	(0.53)
Technology	-		-0.01	(0.04)	1.87***	(0.68)
Transportation, Aerospace	-		0.02*	(0.01)	-0.72	(0.49)
Utilities	-		-0.04***	(0.02)	0.26	(0.33)
Firm-Level						
ln (Mkt. Value)	-0.47*	(0.26)	-0.47*	(0.27)	-	
ln (Sales)	-0.21	(0.28)	-0.25	(0.29)	-	
EpS	-0.06	(0.05)	-0.06	(0.05)	-	
N	483		483			
R^2	0.09		0.10			
F	2.04*		1.47			

Notes: Event window is August 29 - September 9. Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.
The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: BEC, BDK, BKC, CAL, DTV, EKDKQ, ETN, FO, GENZ, HEW.Z, KFT, LSTZA, MI., Q, RGC, SII, and THC.

2.5 Discussion and Conclusion

In this article, I analyzed the stock market response to three distinct ecological disasters. I investigated whether ecological disasters have market-wide impacts. I took advantage of variation in the environmental impact score of publicly listed companies. In my event studies, I found weak evidence that a company's environmental performance can significantly impact its stock market performance following an ecological disaster even in unrelated industries. Further, I found that stock market reactions differ based on the cause of the ecological disaster. The BP oil spill in 2010 had intra-industry and outside-industry spillover effects. Relatively more "green" companies fared better than "brown" companies in most of the affected industries. However, the evidence is mixed. Companies with lower environmental performance scores experienced relatively higher cumulative abnormal returns mainly in some industrial sectors. The stock market response was strongest following the landfall of the oil spill which caused pollution of beaches and losses of marine habitats and wildlife. Similarly, the greenhouse gas emissions following the Iceland volcano eruptions in 2010 significantly affected stock market values. Companies' cumulative abnormal returns differed based on their environmental performances. Again, the results are ambiguous. While environmental performance was positively correlated with cumulative abnormal returns in most industries with significant abnormal returns, health care companies with lower environmental performance scores had relatively higher abnormal returns than health care companies with higher performance scores.

In general, the findings suggest that losses in environmental quality can affect the extent of stock market reactions. However, differences in companies' environmental impacts cannot explain variation in cumulative abnormal returns following Hurricane Irene in 2011. Irene was a natural disaster that may be attributable to climate change. This suggests that consumers and investors are willing to compensate more for environmental damages, in particular for those caused by humans, and either do not associate natural ecological disasters with human-caused climate change or feel less responsible for the consequences (which is in line with the findings of Bulte et al. (2005)). Although the results are event-specific, this study provides some evidence of the importance of corporate environmental performance. The findings indicate that companies' environmental performance matters to investors which supports previous findings (Lyon and Shimshack, 2012). Given that the findings are somewhat ambiguous, possible future avenues include exploration of alternative and more detailed measures of companies' environmental performance.

Chapter 3

Uncertainty and the WTA-WTP Disparity for Private and Public Goods

3.1 Introduction

Abundant empirical evidence from stated preference studies and experiments suggests willingness to accept (WTA) significantly exceeds willingness to pay (WTP) for quantity and price changes. Empirical studies report this disparity for goods ranging from ordinary private goods to public goods (see Horowitz and Mcconnell (2002) for an overview). The observed gap is particularly large for public goods which has important implications for public policies that target reductions in health and safety risk. The disparity also raises questions about the validity of methods used to estimate WTA and WTP. Standard economic theory argues that WTA measures should equal WTP measures for ordinary private goods apart from income and substitution effects (Willig, 1976; Hanemann, 1991). For monetary lotteries however, the welfare measures can differ if individuals' risk preferences are not risk neutral. Alternative theories suggest that individuals' asymmetric risk preferences relative to some reference point can explain the disparity in some settings (e.g. Koszegi and Rabin (2006)). However, economic theory typically cannot explain the magnitude of the disparity or only applies to few contexts. A large WTA-WTP disparity has serious welfare implications: initial allocation of property rights matters substantially and Kaldor-Hicks compensation is no longer direction-neutral. Policy makers widely rely on WTP estimates to assess the value of non-market resources. However, using WTP estimates to establish compensation for environmental damages etc. may be flawed. Importantly, to date, existing empirical and theoretical work paid little attention to risk and uncertainty in the context of public goods. However, uncertainty is of great importance for public policies that address risk reductions related to health and the environment. The benefits of outcomes can vary across individuals or individuals may perceive them differently.

In this study, I analyze the impact of uncertainty on the WTA-WTP disparity using a set of incentive compatible laboratory experiments over money lotteries. The experiments differ from previous work in that they provide a rigorous investigation of the effect of uncertainty on the WTA-WTP disparity in the context of private goods and public goods. In particular, the decision tasks involve scenarios where subjects move from one state of uncertainty to another. This allows me to test whether risk reductions and risk increases affect WTA and WTP differently. Existing lottery experiments mostly involve risk increases and do not involve background risk. I also test whether individuals have social preferences over relative gains and losses in both expected values and risk changes in a public good setting.

In a recent study, Plott and Zeiler (2005) (PZ) argue that experimental confounds may explain anomalies in previous empirical studies. Using an incentive compatible mechanism and avoiding "subject misconceptions", the authors show that the disparity disappears for ordinary private goods. However, following PZ elicitation procedures, Isoni et al. (2011) observe a WTA-WTP disparity for private lotteries over money. Other experimental studies with private lotteries find similar results (Harless, 1989; Eisenberger and Weber, 1995; Neilson et al., 2008; Schmidt and Traub, 2009). However, to date, there exists no comprehensive experimental study that addresses the impact of uncertainty on the WTP-WTA disparity in the context of public goods. Focusing on the importance of uncertainty, I study the effects of social efficiency motives. This is important in the context of public programs where respondents' benefits can differ widely (Messer et al., 2010). Further, public programs typically involve risk or uncertainty in that they present individuals with policy lotteries over uncertain outcomes. In the experiments, subjects make several buying or selling decisions for private lotteries and public good lotteries. In the public good tasks, subjects are matched with one other person and make a decision that affects both their own earnings and the earnings of the matched person. The matched person cannot influence the outcome of the decision. The treatments vary the level of uncertainty and heterogeneity of public good lotteries across individuals which captures inherent differences that are characteristic for public programs.

In a recent working paper, Pahlke et al. (2012) study situations in which an individual makes risky choices that affect the outcome of others. The authors find that individuals display more risk aversion for gains and more risk seeking for losses when being responsible for another person. However, importantly, their experimental setup does not resemble a public program scenario: subjects are either decision makers or passive decision takers. The experimental design in this present study creates a public good setting in the lab and it also has the advantage of eliciting WTA (WTP) directly (i.e. elicitation of point values). In my experimental results, I find strong evidence of a positive WTA-WTP disparity in the uncertainty tasks. This disparity increases in the public good tasks. The findings from the heterogeneous public good tasks suggest that individuals not only have social efficiency preferences but also social risk preferences.

This article proceeds as follows. In section 3.2, I lay out a theoretical framework and derive testable hypotheses. Section 3.3 describes the experimental design and method. I present the results in section 3.4. In section 3.5, I conclude and discuss the main findings.

3.2 Theoretical Framework

In the following section, I derive theoretical predictions with respect to uncertainty for private lotteries following the notation of Abouda (2008). The theory differs from previous work in that it considers scenarios where the decision maker's initial endowment may be a risky asset. The risk of the proposed alternative may therefore be greater than (risk increase) or less than (risk decrease) the initial endowment. I show that the WTA-WTP disparity depends on the decision maker's degree of absolute risk aversion. Then, I discuss the impact of social preferences under certainty (risk-neutrality).

3.2.1 Private Goods

Let $\succeq$ be the individual's transitive and complete preference relation over an asset ξ which satisfies first-order stochastic dominance, continuity, and monotonicity. Consider an individual with property rights to an initial asset, ε, who considers buying an alternative asset α. Both initial and alternative assets can be lotteries or degenerate lotteries. Let $w \in \mathbb{R}$ denote the individual's initial wealth level.

Definition 1. The bid, $b(w;\varepsilon,\alpha)$, of asset α is the buying price such that $w+\varepsilon \sim w-b(w;\varepsilon,\alpha)+\alpha$.

The bid, $b(w;\varepsilon,\alpha)$, is the individual's compensating surplus. Next, assume an individual with property rights to the alternative asset, α, who considers selling the alternative asset to own instead the initial asset, ε.

Definition 2. The offer, $o(w;\varepsilon,\alpha)$, of asset α is the selling price such that $w+\alpha \sim w+o(w;\varepsilon,\alpha)+\varepsilon$.

Offer $o(w;\varepsilon,\alpha)$ is the individual's equivalent surplus. The following proposition establishes a relationship between a parallel bid and offer, which reflects the income effect (Willig, 1976).[1]

Proposition 1. $b(w;\varepsilon,\alpha) = o(w-b(w;\varepsilon,\alpha);\varepsilon,\alpha)$ *and*
$o(w;\varepsilon,\alpha) = b(w+o(w;\varepsilon,\alpha);\varepsilon,\alpha)$.

Proof. Definition 2 gives $w-b(w;\varepsilon,\alpha)+\alpha \sim w-b(w;\varepsilon,\alpha)+o(w-b(w;\varepsilon,\alpha);\varepsilon,\alpha)+\varepsilon$, and by Definition 1 $w+\varepsilon \sim w-b(\omega;\varepsilon,\alpha)+\alpha$, thus $w+\varepsilon \sim w-b(w;\varepsilon,\alpha)+o(w-b(w;\varepsilon,\alpha),\alpha)+\varepsilon$; in order for this to hold, it must be that $b(w;\varepsilon,\alpha) = o(w-b(w;\varepsilon,\alpha);\varepsilon,\alpha)$. Likewise, Definition 1 gives $w+o(w;\varepsilon,\alpha)+\varepsilon \sim w+o(w;\varepsilon,\alpha)-$

[1]Substititution effects do not matter in this setting as goods are lotteries with perfect substitutes.

$b(w+o(w;\varepsilon,\alpha);\varepsilon,\alpha)+\alpha$, and by Definition 2 $w+\alpha\sim w+o(w;\varepsilon,\alpha)+\varepsilon$, and therefore $w+\alpha\sim w+o(w;\varepsilon,\alpha)-b(w+o(w;\varepsilon,\alpha);\varepsilon,\alpha)+\alpha$; it follows that $o(w;\varepsilon,\alpha)=b(w+o(w;\varepsilon,\alpha);\varepsilon,\alpha)$. $\square$

Definition 3. (Pratt, 1964) An individual is weakly risk averse if she always prefers the expected value of an asset to the asset itself, $E[\xi]\succeq\xi,\ \forall\xi$,

where $E[\cdot]$ denotes the expectation operator.

Definition 4. An individual is risk neutral if $E[\xi]\sim\xi,\ \forall\xi$.

Next, I define the risk premium so that I can quantify the impact of the income effect.

Definition 5. (Pratt, 1964) The risk premium, $\pi(w,\xi)$, of asset ξ is the money amount such that $w+\xi\sim w+E[\xi]-\pi(w,\xi)$.

The risk premium is the maximum amount the individual is willing to pay to own the expected value of the asset instead of the asset itself, given her initital level of wealth, w.

Definition 6. (Pratt, 1964) The certainty equivalent, $c(\xi)$, of asset ξ is the money amount such that $w+\xi\sim w+c(\xi)$.

It follows that an individual with weakly risk averse preferences has a positive risk premium, $\pi(\cdot)\geq 0$, as $w+E[\xi]\geq w+c(\xi)\,,\ \forall\xi,\forall w\in\mathbb{R}$.[2] A risk neutral individual's risk premium is equal to zero. Also, the risk premium is an increasing function in risk. Next, I define measures of absolute risk aversion following Pratt (1964).

Definition 7. An individual's risk preferences satisfy constant absolute risk aversion (CARA) if $w+t+\xi\sim w+t+c(\xi)\,,\ \forall t\in\mathbb{R}$, $\forall\xi$,

[2] $\pi(w,\xi)=0$ for all degenerate lotteries, ξ, as $c(\xi)=E[\xi]$.

Definition 8. An individual's risk preferences satisfy DARA if $w + t + \xi \succ w + t + c(\xi)\,,\ \forall t > 0,\, \forall \xi$.

Next, I derive expressions for the optimal bid and offer functions, $b^*(w; \varepsilon, \alpha)$ and $o^*(w; \varepsilon, \alpha)$ in terms of the risk premium. Let $\mu^\xi = E[\xi]$ and $\sigma^\xi = \sqrt{Var[\xi]}$ denote the expected value and standard deviation of an asset ξ, respectively. Define $\Delta\mu \equiv \mu^\alpha - \mu^\varepsilon$ as the difference in expected value between the initial asset and the alternative asset. Assuming independence of outcomes between assets ε and α, define $\Delta\sigma \equiv \sigma^\alpha - \sigma^\varepsilon$ as the difference in standard deviations. I can express the expected value and standard deviation of the alternative asset in terms of μ^ε and σ^ε as follows

$$\mu^\alpha = \mu^\varepsilon + \Delta\mu \tag{3.2.1}$$

$$\sigma^\alpha = \sigma^\varepsilon + \Delta\sigma. \tag{3.2.2}$$

In the following derivations, I only consider positive differences in expected value, $\Delta\mu \geq 0$ (expected gains in WTP).

Willingness to Pay

The risk premium for asset ε is, given initial wealth w, is

$$w + \varepsilon \sim w + \mu^\varepsilon - \pi(w, \varepsilon)\,. \tag{3.2.3}$$

Using Definition 1 this is equivalent to

$$w + \mu^\varepsilon - \pi(w, \varepsilon) \sim w - b(w; \varepsilon, \alpha) + \alpha. \tag{3.2.4}$$

The risk premium for asset α, given initial wealth $w - b\,(w;\varepsilon,\alpha)$, is the amount such that

$$w - b\,(w;\varepsilon,\alpha) + \alpha \sim w - b\,(w;\varepsilon,\alpha) + (\mu^{\varepsilon} + \varDelta\mu) - \pi\,(w - b\,(w;\varepsilon,\alpha)\,,\alpha)\,. \quad (3.2.5)$$

Comparing expression 3.2.4 to expression 3.2.5 gives

$$w + \mu^{\varepsilon} - \pi\,(w,\varepsilon) \sim w - b\,(w;\varepsilon,\alpha) + (\mu^{\varepsilon} + \varDelta\mu) - \pi\,(w - b\,(w;\varepsilon,\alpha)\,,\alpha)\,. \quad (3.2.6)$$

For this to hold, it must be that

$$-\,\pi\,(w,\varepsilon) = \varDelta\mu - b\,(w;\varepsilon,\alpha) - \pi\,(w - b\,(w;\varepsilon,\alpha)\,,\alpha)\,. \quad (3.2.7)$$

Solving for $b\,(\cdot)$ gives the optimal bid

$$b^{*} = \varDelta\mu + [\pi\,(w,\varepsilon) - \pi\,(w - b\,(w;\varepsilon,\alpha)\,,\alpha)]\,. \quad (3.2.8)$$

Expression 3.2.8 states that the individual's bid (maximum buying price) is the sum of the difference in expected payouts, $\varDelta\mu$, and the difference in risk premia between the initial and the alternative asset. For an individual with risk neutral preferences, the bid is simply the difference in expected payouts, $\varDelta\mu$, since $\pi\,(w,\xi) = 0,\ \forall w \in \mathbb{R}, \forall\xi$. If the alternative asset is a degenerate lottery, then equation 3.2.8 simplifies to

$$b^{*}\,(w;\varepsilon,\alpha) = \varDelta\mu + \pi\,(w,\varepsilon)\,. \quad (3.2.9)$$

A weakly risk-averse individual increases her bid above the difference in expected payouts to avoid the risk of the initial asset and own the certain alternative instead.

Conversely, if the initial asset is a degenerate lottery, she decreases her bid below the difference in expected payouts:

$$b^*(w;\varepsilon,\alpha) = \varDelta\mu - \pi(w - b^*(w;\varepsilon,\alpha),\alpha). \tag{3.2.10}$$

If both assets are non-degenerate lotteries, the invididual's bid can be less than or greater than the difference in expected payouts.

In general, the optimal bid of a weakly risk averse individual decreases (increases) as the relative risk increases (decreases). Also, the increase in the bid associated with a risk decrease is smaller in magnitude than the decrease in the bid for an equivalent risk increase. Let ε, α_1, α_2 denote assets such that $\mu^{\alpha_1} = \mu^{\alpha_2} > \mu^{\varepsilon}$, $\sigma^{\alpha_1} > \sigma^{\varepsilon} > \sigma^{\alpha_2}$ where $(\sigma^{\alpha_1} - \sigma^{\varepsilon}) = (\sigma^{\varepsilon} - \sigma^{\alpha_2}) > 0$. It follows from equation 3.2.8 that $b^*(w;\varepsilon,\alpha_1) - b^*(w;\varepsilon,\alpha_2) = -\pi(w - b^*(w;\varepsilon,\alpha_1),\alpha_1) + \pi(w - b^*(w;\varepsilon,\alpha_2),\alpha_2)$. This difference is less than zero for a weakly risk-averse individual with CARA or DARA as the risk premium is an increasing function in risk. Note that this asymmetry is greater for an individual with DARA as the risk premium is decreasing in wealth.

Willingness to Accept

To derive an expression for the optimal offer (maximum selling price), I express the optimal bid function in terms of the optimal offer.[3] Let $w + o^*(w;\varepsilon,\alpha)$ denote the inititial wealth, then expression 3.2.8 becomes

$$\begin{aligned} b^*(\omega + o^*(w;\varepsilon,\alpha);\varepsilon,\alpha) &= \varDelta\mu + \pi(w + o^*(w;\varepsilon,\alpha),\varepsilon) \\ &- \pi(w - b(w + o^*(w;\varepsilon,\alpha);\varepsilon,\alpha) + o^*(w;\varepsilon,\alpha),\alpha). \end{aligned} \tag{3.2.11}$$

[3] Alternatively, I can use definition 2 and definition 5 to derive the optimal offer.

Following proposition 1, this simplifies to

$$o^*(w;\varepsilon,\alpha) = \Delta\mu + \left[\pi\left(w + o^*(w;\varepsilon,\alpha),\varepsilon\right) - \pi(w,\alpha)\right]. \tag{3.2.12}$$

Expression 3.2.12 states that the individual's offer (maximum selling price) is the sum of the difference in expected payouts, $\Delta\mu$, and the difference in risk premia between the initial asset and the alternative asset. For an individual with risk neutral preferences, the offer is simply the difference in expected payouts, $\Delta\mu$, since $\pi(w,\xi) = 0,\ \forall w \in \mathbb{R}, \forall\xi$. If the alternative is a degenerate lottery, then equation 3.2.12 simplifies to

$$o^*(w;\varepsilon,\alpha) = \Delta\mu + \pi\left(w + o^*(w;\varepsilon,\alpha),\varepsilon\right) \tag{3.2.13}$$

A weakly risk-averse utility maximizer increases her offer above the difference in expected payouts to obtain the risky initial asset instead of the certain alternative. Conversely, if the initial asset is a degenerate lottery, she decreases her offer below the difference in expected payouts to obtain the certain initial asset and part with the risky alternative:

$$o^*(w;\varepsilon,\alpha) = \Delta\mu - \pi(w,\alpha) \tag{3.2.14}$$

If both assets are non-degenerate lotteries, the invididual's offer can be less than or greater than the difference in expected payouts.

In general, the optimal offer of a weakly risk-averse individual decreases (increases) as the relative risk increases (decreases). Also, the increase in the bid associated with a risk decrease is smaller in magnitude than the decrease in the bid for an equivalent risk increase. Let ε_1, ε_2, α denote assets such that $\mu^{\varepsilon_1} = \mu^{\varepsilon_2} < \mu^{\alpha}$, $\sigma^{\varepsilon_1} >$

$\sigma^{\alpha} > \sigma^{\varepsilon_2}$ where $(\sigma^{\alpha} - \sigma^{\varepsilon_1}) = (\sigma^{\varepsilon_2} - \sigma^{\alpha}) < 0$. It follows from equation 3.2.12 that $o^*(w;\varepsilon_1,\alpha) - o^*(w;\varepsilon_2,\alpha) = \pi(w + o^*(w;\varepsilon_1,\alpha),\varepsilon_1) - \pi(w + o^*(w;\varepsilon_2,\alpha),\varepsilon_2)$. This difference is greater than zero for a weakly risk-averse individual with CARA or DARA as the risk premium is an increasing function in risk. Note that this asymmetry is greater for an individual with DARA as the risk premium is decreasing in wealth.

WTP and WTA Disparities

Substracting the optimal bid (WTP) from the optimal offer (WTA) gives

$$\begin{aligned} o^*(w;\varepsilon,\alpha) - b^*(w;\varepsilon,\alpha) &= [\pi(w + o(w;\varepsilon,\alpha),\varepsilon) - \pi(w,\varepsilon)] \\ &+ [\pi(w - b(w;\varepsilon,\alpha),\alpha) - \pi(w,\alpha)]. \end{aligned} \tag{3.2.15}$$

For a risk neutral individual this difference is equal to zero.

Proposition 2. *WTA equals WTP for an individual with risk neutral preferences,* $o^*(w;\varepsilon,\alpha) - b^*(w;\varepsilon,\alpha) = 0$.

Proof. Risk neutrality implies $\pi(w,\xi) = 0,\ \forall w \in \mathbb{R}, \forall \xi$, and therefore $o^*(w;\varepsilon,\alpha) - b^*(w;\varepsilon,\alpha) = 0$. □

Proposition 3. *If the individual's risk preferences satisfy CARA then* $o^*(w;\varepsilon,\alpha) - b^*(w;\varepsilon,\alpha) = 0$.

Proof. One can show that if an individual's risk preferences satisfy CARA then her risk premium is constant in income. Then, $\pi(w + o(\cdot),\varepsilon) - \pi(w,\varepsilon) = 0$ and $\pi(w,\alpha) - \pi(w - b(\cdot),\alpha) = 0$ and, thus $b^*(\cdot) - o^*(\cdot) = 0$. □

Proposition 4. *The following assertions are equivalent:*

(i) The optimal offer is less than the optimal bid for any assets ε, α *that satisfy* $\mu^{\alpha} \geq \mu^{\varepsilon}$, $\sigma^{\varepsilon} > 0$, $\sigma^{\alpha} = 0$*:* $o^{*}(w; \varepsilon, \alpha) - b^{*}(w; \varepsilon, \alpha) < 0$ *and*

the optimal offer is greater than the optimal bid for any assets ε, α *that satisfy* $\mu^{\alpha} \geq \mu^{\varepsilon}$, $\sigma^{\varepsilon} = 0$, $\sigma^{\alpha} > 0$*:* $o^{*}(w; \varepsilon, \alpha) - b^{*}(w; \varepsilon, \alpha) > 0$.

(ii) The individual's risk preferences satisfy DARA.

Proof. (i) $\Rightarrow$ (ii)

Case 1: $(\sigma^{\varepsilon} > 0,\ \sigma^{\alpha} = 0)$ $o^{*}(\cdot) - b^{*}(\cdot) = \pi(w + o^{*}(\cdot), \varepsilon) - \pi(w, \varepsilon) < 0$, implies that the risk premium is a decreasing function of wealth. One can show that a risk premium that is decreasing in wealth is equivalent to the individual having DARA risk preferences (see Abouda (2008) for proof).

Case 2: $(\sigma^{\varepsilon} = 0,\ \sigma^{\alpha} > 0)$ $o^{*}(\cdot) - b^{*}(\cdot) = \pi(w, \alpha) - \pi(w - b^{*}(\cdot), \alpha) > 0$, implies that the risk premium is a decreasing function of wealth, which is equivalent to DARA risk preferences.

(ii) $\Rightarrow$ (i) If the invidiual satifies DARA then the risk premium is a decreasing function of wealth.

Case 1: $(\sigma^{\varepsilon} > 0,\ \sigma^{\alpha} = 0)$ $o^{*}(\cdot) - b^{*}(\cdot) = \pi(w + o^{*}(\cdot), \varepsilon) - \pi(w, \varepsilon) < 0$.

Case 2: $(\sigma^{\varepsilon} = 0,\ \sigma^{\alpha} > 0)$ $o^{*}(\cdot) - b^{*}(\cdot) = \pi(w, -b^{*}(\cdot), \alpha) - \pi(w, \alpha) > 0$. $\square$

The general case, where neither the initial asset nor the alternative are degenerate lotteries, is ambiguous under DARA. I will use examples within the expected utility framework to illustrate the disparity between WTA and WTP when both assets are uncertain. First, consider a pure risk decrease in WTP.[4]

[4]Davis and Reilly (2012) mention the possibility of a negative WTA-WTP disparity with uncertainty for changes in quantity.

Example 1. Set $w = \$0$ and let $\varepsilon_1 = [\$0, \$8; 0.5, 0.5]$ and $\alpha_1 = [\$2, \$6; 0.5, 0.5]$ ($\mu^{\varepsilon_1} = \mu^{\alpha_1} = \$4$, $\sigma^{\varepsilon_1} = \$4 > \$2 = \sigma^{\alpha_1}$). Further let the individual's Bernoulli utility function be $u(x) = x^{1-r}/(1-r)$, $x \in \mathbb{R}$ with $r = 0.53$[5] (this utility function satisfies DARA). The optimal bid, $b(\$0; \varepsilon_1, \alpha_1) = \$1.60$, is greater than the corresponding offer, $o(\$0; \varepsilon_1, \alpha_1) = \$0.84$.

Next, consider a relative risk increase in WTP[6].

Example 2. Set $w = \$0$ and let $\varepsilon_2 = [\$2, \$6; 0.5, 0.5]$ and $\alpha_2 = [\$2, \$10; 0.5, 0.5]$ ($\mu^{\varepsilon_2} = \$4$, $\mu^{\alpha_2} = \$6$, $\sigma^{\varepsilon_2} = \$2 < \$4 = \sigma^{\alpha_2}$). Again, let the individual's Bernoulli utility function be $u(x) = x^{1-r}/(1-r)$, $x \in \mathbb{R}$ with $r = 0.53$. The optimal bid, $b(\$0; \varepsilon_2, \alpha_2) = \$1.16$, is less than the corresponding offer, $o(\$0; \varepsilon_2, \alpha_2) = \$1.39$.

3.2.2 Public Goods

There are several theoretical frameworks that model an individual's behavior toward others. Here, I discuss the widely-applied models of Charness and Rabin (2002) and Fehr and Schmidt (1999). Charness and Rabin (2002) assume that individuals' preferences are driven by social efficiency concerns and that inidividuals want to maximize the welfare of the worst-off individual. Fehr and Schmidt (1999) assume that individuals are averse to inequality among individuals (the inequality model of Bolton and Ockenfels (2000) gives similar predictions). Note that none of the models assume uncertainty. Therefore, I express the bid and offer functions in terms of expected gains only. In the following derivations, assume for simplicity that $w_1 = w_2 = 0$.

[5] Parameter estimate from Harrison and Rutström (2008).

[6] Note that a pure risk increase in WTP is equivalent to short buying; however, in this study I only consider gains in WTP.

Social Efficiency

Consider an individual with property rights to asset an asset, ε_1, who considers buying an alternative asset, α_1. Importantly, her decision now also affects whether a second individual with property rights to asset, ε_2, will buy an alternative asset, α_2. That is, the first individual makes a pivotal buying decision. Let $p = [\varepsilon_1, \alpha_1; \varepsilon_2, \alpha_2] = p[\mathit{\Delta}\mu_1, \mathit{\Delta}\mu_2]$ denote this *public program*, where $\mathit{\Delta}\mu_i = (\mu^{\alpha_i} - \mu^{\varepsilon_i})$, $i = 1, 2$. Let the decision making individual's utility function be of the following form $u(\mathbf{x}) = u(x_1 + \lambda x_2)$, where $\lambda \geq 0$ denotes the level of altruism toward the other individual (Charness and Rabin, 2002).

Definition 9. The socially efficient bid, $b_s(p)$, of public program p is the buying price such that $u(\mu^{\varepsilon_1} + \lambda\mu^{\varepsilon_2}) = u(\mu^{\alpha_1} - b_s(p) + \lambda(\mu^{\alpha_2} - b_s(p)))$.

The socially efficient bid can be written as (applying the inverse of the utility function and solving for $b_s(p)$)

$$b_s^*(p) = \frac{\mathit{\Delta}\mu_1 + \lambda\mathit{\Delta}\mu_2}{1+\lambda}. \tag{3.2.16}$$

Next, consider an individual with property rights to asset, α_1, who considers selling the alternative asset to own the asset, ε_1, instead. Importantly, her decision now also affects whether a second individual with proptery right to asset, α_2, will sell asset α_2 to own asset ε_2 instead (pivotal selling decision).

Definition 10. The socially efficient offer, $o_s(p)$, of public program p is the selling price such that $u(\mu^{\alpha_1} + \lambda\mu^{\alpha_2}) = u(\mu^{\varepsilon_1} + o_s(p) + \lambda(\mu^{\varepsilon_2} + o_s(p)))$.

The socially efficient offer can be written as (applying the inverse of the utility function and solving for $o\left(p\right)$)

$$o_s^*\left(p\right) = \frac{\Delta\mu_1 + \lambda\Delta\mu_2}{1+\lambda}, \tag{3.2.17}$$

which is equal to the socially efficient bid. The bid (offer) increases in the other person's relative expected gain.

Maximin Preferences

The bid and offer functions for maximin preferences have the same form as the socially efficient bid function if the other individual's payoffs (initial and alternative) are less than the decision making individual's payoffs, $\mu^{\varepsilon_2} < \mu^{\varepsilon_1}$ and $\mu^{\alpha_2} < \mu^{\alpha_1}$. If the other person's payoffs are greater, then the individual's bid (offer) will be equal to her private bid (offer). However, there can be a disparity between bid and offer if the other person is not always worse off:

$$b_m^*\left(p\right) = \begin{cases} \Delta\mu_1 - \kappa\cdot\mu^{\varepsilon_2} & : \mu^{\varepsilon_2} < \mu^{\varepsilon_1},\ \mu^{\alpha_2} \geq \mu^{\alpha_1} \\ \left(\Delta\mu_1 + \kappa\cdot\mu^{\alpha_2}\right)/\left(1+\kappa\right) & : \mu^{\varepsilon_2} \geq \mu^{\varepsilon_1},\ \mu^{\alpha_2} < \mu^{\alpha_1} \end{cases} \tag{3.2.18}$$

and

$$o_m^*\left(p\right) = \begin{cases} \left(\Delta\mu_1 - \kappa\cdot\mu^{\varepsilon_2}\right)/\left(1+\kappa\right) & : \mu^{\varepsilon_2} < \mu^{\varepsilon_1},\ \mu^{\alpha_2} \geq \mu^{\alpha_1} \\ \Delta\mu_1 + \kappa\cdot\mu^{\alpha_2} & : \mu^{\varepsilon_2} \geq \mu^{\varepsilon_1},\ \mu^{\alpha_2} < \mu^{\alpha_1} \end{cases}, \tag{3.2.19}$$

where $\kappa \geq 0$. This implies that $o_m^*\left(p\right) \geq b_m^*\left(p\right)$ if $\mu^{\varepsilon_2} \geq \mu^{\varepsilon_1}$, $\mu^{\alpha_2} < \mu^{\alpha_1}$ and $o_m^*\left(p\right) \leq b_m^*\left(p\right)$ if $\mu^{\varepsilon_2} < \mu^{\varepsilon_1}$, $\mu^{\alpha_2} \geq \mu^{\alpha_1}$. That is, heterogeneity in initial payouts can yield a negative WTA-WTP disparity if the other person's initial payout is less. Similarly, heterogeneity in alternative payouts can yield a positive WTA-WTP disparity if the other person's alternative payout is less. In general, with heterogeneous payouts, the bid (offer) will differ from the difference in the individual's expected payout. Note that quasi-maximin preferences (Charness and Rabin, 2002) are a combination of social efficiency and maximin preferences that result in a similar disparity if the other person is not always worse off.

Fairness

Let the individual's utility function be of the following form $u_1(\mathbf{x}) = x_1 - \zeta \cdot \max\{0,\, x_2 - x_1\} - \eta \cdot \max\{0,\, x_1 - x_2\}$, where $\eta \leq \zeta$ and $\zeta \in [0,\, 1)$ (Fehr and Schmidt, 1999). The individual gets disutility if her own payoff differs from the other person's payoff. The disutility is greater when the individual's payoff is less than the other person's payoff compared to when her payoff is greater.

Definition 11. The fair bid, $b_f(p)$, of public program p is the buying price such that $\mu^{\varepsilon_1} - \zeta \cdot \max\{0,\, \mu^{\varepsilon_2} - \mu^{\varepsilon_1}\} - \eta \cdot \max\{0,\, \mu^{\varepsilon_1} - \mu^{\varepsilon_2}\} = \mu^{\alpha_1} - b_f(p) - \zeta \cdot \max\{0,\, \mu^{\alpha_2} - \mu^{\alpha_1}\} - \eta \cdot \max\{0,\, \mu^{\alpha_1} - \mu^{\alpha_2}\}$.

Let $\delta\mu^{\alpha} = \mu^{\alpha_2} - \mu^{\alpha_1}$ and $\delta\mu^{\varepsilon} = \mu^{\varepsilon_2} - \mu^{\varepsilon_1}$. The fair bid can then be written as

$$\begin{aligned} b_f^*(p) &= \varDelta\mu_1 - \zeta \cdot (\max\{0,\, \delta\mu^{\alpha}\} - \max\{0,\, \delta\mu^{\varepsilon}\}) \\ &- \eta \cdot (\min\{0,\, \delta\mu^{\alpha}\} - \min\{0,\, \delta\mu^{\varepsilon}\})\,. \end{aligned} \tag{3.2.20}$$

Definition 12. The fair offer, $o_f(p)$, of public program p is the selling price such that$w_1 + \mu^{\alpha_1} - \zeta \cdot \max\{0,\, \mu^{\alpha_2} - \mu^{\alpha_1}\} - \eta \cdot \max\{0,\, \mu^{\alpha_1} - \mu^{\alpha_2}\} = w_1 + \mu^{\varepsilon_1} + o_f(p) - \zeta \cdot \max\{0,\, \mu^{\varepsilon_2} - \mu^{\varepsilon_1}\} - \eta \cdot \max\{0,\, \mu^{\varepsilon_1} - \mu^{\varepsilon_2}\}$.

The fair offer can be written as

$$o_f^*(p) = \Delta\mu_1 - \zeta\cdot(\max\{0,\,\delta\mu^{\alpha}\} - \max\{0,\,\delta\mu^{\varepsilon}\}) \qquad (3.2.21)$$
$$- \eta\cdot(\min\{0,\,\delta\mu^{\alpha}\} - \min\{0,\,\delta\mu^{\varepsilon}\}),$$

which is equivalent to the fair bid. Here, heterogeneous payouts across individuals can result in asymmetric increases or decreases in bids (offers).

Example 3. Let $\mu^{\varepsilon_1} = \mu^{\varepsilon_2} = \$4$ and $\mu^{\alpha_1} = \$8$, $\mu^{\alpha_2} = \$6$ ($\Delta\mu_1 = \$2$, $\delta\mu^{\varepsilon} = \$0$ and $\delta\mu^{\alpha} = -\$2$). The fair bid is $b_f^* = o_f^* = \$2 - \eta\cdot\$2 < \$2$, $\eta > 0$.

Example 4. Let $\mu^{\varepsilon_1} = \mu^{\varepsilon_2} = \$4$ and $\mu^{\alpha_1} = \$8$, $\mu^{\alpha_2} = \$6$ ($\Delta\mu_1 = \$2$, $\delta\mu^{\varepsilon} = \$0$ and $\delta\mu^{\alpha} = \$2$). The fair bid is $b_f^* = o_f^* = \$2 - \zeta\cdot\$2 \leq \$2 - \eta\cdot\$2 < \$2$, $\zeta \geq \eta > 0$.

To summarize, neither social efficiency nor fairness predict a WTA-WTP disparity. Maximin preferences (and quasi-maximin preferences) predict a positive (negative) WTA-WTP disparity only if the other person is worse off with the initial (alternative) payoffs.

3.3 Experimental Design and Hypotheses

The experimental design consists of two main conditions: (1) WTA and (2) WTP (between subject design). In each condition, participants first obtain property rights to an asset which is either a sure amount of money or a symmetric, binary lottery (50-50 chance). In WTA, participants then have the opportunity to sell their property rights and obtain rights to another asset instead (sure amount of money or 50-50 lottery). If they sell, they give up their inital property rights and receive a selling price for the transaction. In WTP, participants have the opportunity to purchase property rights to an alternative asset. If they purchase, they give up their property rights to their initial asset to obtain the alternative instead and pay a buying price for the transaction. Within each of these two main treatments, the experiment consists of three parts (within-subject design within each main treatment):

1. **Training Tasks:** Participants make six individual buying (selling) decisions over certain, low stakes payouts (i.e. degenerate lotteries).

2. **Private Good Tasks:** Participants make eight individual buying (selling) decisions over high stakes payouts and high stakes lotteries. The individual high stakes tasks vary the difference in expected payout, $\Delta\mu$, and the difference in standard deviation between initial and alternative asset, $\Delta\sigma$.

3. **Public Good Tasks:** Participants make a total of 23 group buying (selling) decisions over high stakes payouts and high stakes lotteries in groups of two (Public Good tasks). The bids (offers) affect not only the decision maker but also a second person. Importantly, the other person has no opportunity to affect the outcome of a buying (selling) decision. The group high stakes tasks vary the difference in expected payout, $\Delta\mu$, and the difference in standard deviation between initial and alternative asset, $\Delta\sigma$, for both the decision maker, and the other matched person. Participants make decisions on

   (a) eight homogeneous tasks (the decision maker's initial and alternative assets are the same as the other person's assets) and

   (b) 15 heterogeneous tasks (the decision maker's initial and alternative assets are different than the other person's assets).

The private and public good tasks vary expected payouts and levels of uncertainty (standard deviation) between the initial and the alternative asset. Hereafter, I refer to the types of variations as treatments (within-subject). Table 3.1 shows the main treatments.

I use the Becker et al. (1964) (BDM) mechanism to elicit bids (offers). With the BDM mechanism, bids (offers) are compared to a randomly chosen price. In the WTP tasks, if the bid greater than or the same as the randomly chosen price, a buy will take place. In the WTA tasks, if the offer is less than or the same as the randomly chosen price, a sale will take place. In the WTP (WTA) training tasks, the BDM price was drawn from a uniform distribution on the interval $[\$0.01, \$1.10]$ ($[\$0, \$1.09]$) in one cent increments. In the high stakes WTP (WTA) tasks, the random price was drawn from a uniform distribution on the interval $[\$0.01, \$12]$ ($[\$0, \$11.99]$) in one cent increments.

Table 3.1: Treatment Categories by Welfare Change and Risk Change

Treatment	1	2	3	4	5†
$\Delta\mu$	$\oplus$	$\circ$	$\oplus$	$\oplus$	$\oplus$
$\Delta\sigma$	$\circ$	$\ominus$	$\ominus$	$\ominus$	$\oplus\ominus$

† Heterogeneous public good task only

The following subsections provide details on the construction of the assets (lotteries) in the experiment.

3.3.1 Asset Choices

The computer progam generates the initial and alternative assets during a session. In both private good and public good tasks, the assets are either certain payouts or binary lotteries. Both states of any given lottery have a 50% chance of occurring.

The expected value of the initial asset is \$4. The expected value of the alternative asset is in the range of $[\$5, \$12]$ (except for treatment 2 in Table 3.1). The standard deviation of an asset is in the range of $[\$0, \$4]$. For assets where $\sigma \neq 0$, the lower payout is constructed by subtracting the standard deviation from the expected payout. Likewise, the higher payout is constructed by adding the standard deviation to the expected payout. Thus, all risky assets are symmetric, binary lotteries with a spread of 2σ between low and high payout.

3.3.1.1 Training Tasks

In the training tasks, the computer sets the certain payout of the initial asset by randomly drawing a number from a uniform distribution on the interval $[\$0.05, \$0.50]$ in 5 cent increments. For the alternative asset's certain payout, the computer randomly draws a number from a uniform distribution on the interval $[\$0.55, \$1]$, again in 5 cent increments. Note that the intervals of the initial and alternative asset do not overlap.

3.3.1.2 Private Good Tasks

Table 3.2 lists the asset parameters for the private good tasks. In each task, the initial asset (Status Quo in WTP, Alternative in WTA treatment) has an average payout of four dollars. The computer program sets the average payout of the alternative asset (Alternative in WTP, Status Quo in WTA treatment) based on the parameters given in Table 3.2. The computer draws the difference in average payouts between initial and alternative asset from a uniform distribution in one dollar increments (except for tasks 3 and 4). The program then sets the standard deviation of the initial asset, σ^{ε}, according to the parameters and calculates the standard deviation of the alternative asset. The standard deviation values are drawn from a uniform distribution in 50 cent increments. Note that the difference in expected payouts is non-negative in all tasks. The tasks that vary the standard deviation between initial and alternative assets such that $\Delta\sigma < 0$ if $\Delta\mu \geq 0$ and $\Delta\sigma < 0$ only if $\Delta\mu > 0$ (i.e. the alternative is a "good" in that expected payout increases when risk increases). Put differently, none of the tasks are short buying (selling) tasks. In tasks 1 and 2 only the expected payout changes between initial and alternative asset. In tasks 3 and 4 only the standard deviation of the initial and alternative asset changes. In all other tasks, both the expected payout and the standard deviation changes between initial and alternative asset.

Table 3.2: Treatment Parameters, Individual (Homogeneous Group) Tasks

#	Treatment	μ^ε	$\Delta\mu$	σ^ε	$\Delta\sigma$
1	1	\$4	$\sim U[\$1, \$8]$	\$0	\$0
2	1	\$4	$\sim U[\$1, \$8]$	$\sim U[\$0.5, \$4]$	\$0
3	2	\$4	\$0	$\sim U[\$0.5, \$4]$	$-\sigma^\varepsilon$
4	2	\$4	\$0	$\sim U[\$0.5, \$4 - \Delta\sigma] + \Delta\sigma$	$\sim U[\$0.5, \$3.5]$
5	3	\$4	$\sim U[\$1, \$8]$	\$0	$\sim U[\$0.5, \$4]$
6	3	\$4	$\sim U[\$1, \$8]$	$\sim U[\$0.5, \$4 - \Delta\sigma]$	$\sim U[\$0.5, \$3.5]$
7	4	\$4	$\sim U[\$1, \$8]$	$\sim U[\$0.5, \$4]$	$-\sigma^\varepsilon$
8	4	\$4	$\sim U[\$1, \$8]$	$\sim U[\$0.5, \$4 - \Delta\sigma] + \Delta\sigma$	$\sim U[\$0.5, \$3.5]$

Note: Values for $\Delta\mu$ are drawn in \$1 increments. Values for σ^ε ($\Delta\sigma$) are drawn in \$0.50 increments.

3.3.1.3 Public Good Tasks

In eight of the public good tasks, assets are homogeneous across decision maker and other person. The program uses the same algorithm as in the private good tasks laid out above to construct the assets for the homogeneous tasks. In the other 15 public good tasks, assets are heterogeneous across decision maker and other person. Tables 3.3 and 3.4 list the asset parameters (expected payouts and standard deviations) for the heterogeneous public good tasks.

Table 3.3: Expected Payout Parameters, Heterogeneous Group Task Lotteries

#	Treat.	$\mu_1^\varepsilon = \mu_2^\varepsilon$	$d\mu$	$\Delta\mu$	$\Delta\mu_1$	$\Delta\mu_2$
1	1	\$4	$\sim U[\$1, \$7]$	$\sim U[\$0, \$8 - d\mu]$	$\Delta\mu + \delta d\mu$	$\Delta\mu + (1-\delta)\,d\mu$
2	1	\$4	$\sim U[\$1, \$7]$	$\sim U[\$0, \$8 - d\mu]$	$\Delta\mu + d\mu$	$\Delta\mu$
3	1	\$4	$\sim U[\$1, \$7]$	$\sim U[\$0, \$8 - d\mu]$	$\Delta\mu$	$\Delta\mu + d\mu$
4	2	\$4	$\sim U[\$1, \$7]$	-	\$0	\$0
5	2	\$4	-	-	\$0	\$0
6	2	\$4	-	-	\$0	\$0
7	3	\$4	$\sim U[\$1, \$7]$	$\sim U[\$1, \$8 - d\mu]$	$\Delta\mu + \delta d\mu$	$\Delta\mu + (1-\delta)\,d\mu$
8	3	\$4	$\sim U[\$1, \$7]$	$\sim U[\$1, \$8 - d\mu]$	$\Delta\mu + d\mu$	$\Delta\mu$
9	3	\$4	$\sim U[\$1, \$7]$	$\sim U[\$1, \$8 - d\mu]$	$\Delta\mu$	$\Delta\mu + d\mu$
10	4	\$4	$\sim U[\$1, \$7]$	$\sim U[\$0, \$8 - d\mu]$	$\Delta\mu + \delta d\mu$	$\Delta\mu + (1-\delta)\,d\mu$
11	4	\$4	$\sim U[\$1, \$7]$	$\sim U[\$1, \$8 - d\mu]$	$\Delta\mu + d\mu$	$\Delta\mu$
12	4	\$4	$\sim U[\$1, \$7]$	$\sim U[\$0, \$8 - d\mu]$	$\Delta\mu$	$\Delta\mu + d\mu$
13	5	\$4	$\sim U[\$1, \$7]$	$\sim U[\$1, \$8 - d\mu]$	$\Delta\mu + \delta d\mu$	$\Delta\mu + (1-\delta)\,d\mu$
14	5	\$4	$\sim U[\$1, \$7]$	$\sim U[\$1, \$8 - d\mu]$	$\Delta\mu + d\mu$	$\Delta\mu$
15	5	\$4	$\sim U[\$1, \$7]$	$\sim U[\$0, \$8 - d\mu]$	$\Delta\mu$	$\Delta\mu + d\mu$

Notes: Subscripts 1 and 2 denote decision maker and other person, respectively. δ is an indicator function which is either 1 or 0 based on a random draw.

Table 3.4: Standard Deviation Parameters, Heterogeneous Group Task Lotteries

#	Treat.	$d\sigma$	$\Delta\sigma$	σ_1^ε	σ_2^ε	$\Delta\sigma_1$	$\Delta\sigma_2$
1	1	-	-	\$0	\$0	\$0	\$0
2	1	$\sim U[\$0,\$4]$	$\sim U[\$0,\$4-d\sigma]$	$d\sigma+\Delta\sigma$	$d\sigma$	\$0	\$0
3	1	$\sim U[\$0,\$4]$	$\sim U[\$0,\$4-d\sigma]$	$d\sigma$	$d\sigma+\Delta\sigma$	\$0	\$0
4	2	$\sim U[\$0.5,\$4]$	$\sim U[\$0,\$4-d\sigma]$	$(1-\delta)\,d\sigma+\Delta\sigma$	$\delta d\sigma+\Delta\sigma$	$-\sigma_D^\varepsilon$	\$0
5	2	$\sim U[\$0.5,\$4]$	$\sim U[\$0,\$4-d\sigma]$	\$4	\$4	$\sigma_D^\varepsilon-(d\sigma+\Delta\sigma)$	$\Delta\sigma-\sigma_R^\varepsilon$
6	2	$\sim U[\$0.5,\$4]$	$\sim U[\$0,\$4-d\sigma]$	\$4	\$4	$\sigma_D^\varepsilon-\Delta\sigma$	$\sigma_R^\varepsilon-(d\sigma+\Delta\sigma)$
7	3	$\sim U[\$0,\$4]$	$\sim U[\$0,\$4-d\sigma]$	\$0	\$0	$(1-\delta)\,d\sigma+\Delta\sigma$	$\delta d\sigma+\Delta\sigma$
8	3	$\sim U[\$0,\$4]$	$\sim U[\$0,\$4-d\sigma]$	$d\sigma+\Delta\sigma$	$d\sigma$	$\$4-\sigma_D^\varepsilon$	$\$4-\sigma_R^\varepsilon$
9	3	$\sim U[\$0,\$4]$	$\sim U[\$0,\$4-d\sigma]$	$d\sigma$	$d\sigma+\Delta\sigma$	$\$4-\sigma_D^\varepsilon$	$\$4-\sigma_R^\varepsilon$
10	4	$\sim U[\$0,\$4]$	$\sim U[\$0,\$4-d\sigma]$	$(1-\delta)\,d\sigma+\Delta\sigma$	$\delta d\sigma+\Delta\sigma$	$-\sigma_D^\varepsilon$	$-\sigma_R^\varepsilon$
11	4	$\sim U[\$0,\$4]$	$\sim U[\$0,\$4-d\sigma]$	\$4	\$4	$d\sigma+\Delta\sigma-\sigma_D^\varepsilon$	$\Delta\sigma-\sigma_R^\varepsilon$
12	4	$\sim U[\$0,\$4]$	$\sim U[\$0,\$4-d\sigma]$	\$4	\$4	$\Delta\sigma-\sigma_D^\varepsilon$	$d\sigma+\Delta\sigma-\sigma_R^\varepsilon$
13	5	$\sim U[\$0.5,\$4]$	$\sim U[\$0,\$4-d\sigma]$	$(1-\delta)\,d\sigma+\Delta\sigma$	\$0	$-\sigma_D^\varepsilon$	$\delta d\sigma+\Delta\sigma$
14	5	$\sim U[\$0,\$4]$	$\sim U[\$0,\$4-d\sigma]$	\$4	$d\sigma+\Delta\sigma$	$\Delta\sigma-\sigma_D^\varepsilon$	$d\sigma+\Delta\sigma-\sigma_R^\varepsilon$
15	5	$\sim U[\$0,\$4]$	$\sim U[\$0,\$4-d\sigma]$	\$4	$d\sigma$	$d\sigma+\Delta\sigma-\sigma_D^\varepsilon$	$\Delta\sigma-\sigma_R^\varepsilon$

Notes: Subscripts 1 and 2 denote decision maker and other person, respectively. δ is an indicator function which is either 1 or 0 based on a random draw.

3.3.2 Hypotheses

The objective of the experimental design is to test the effect of uncertainty on the WTA-WTP disparity for private good and public good lotteries. Based on the theoretical derivations in section 3.2, I lay out the following experimental hypotheses. The first hypotheses refer to relative risk changes between assets ε and α and are stated independently from private good and public good task. Note that these hypotheses assume that the average individual is weakly risk averse.

Hypothesis 3.1. The decrease in the offer for a relative risk increase, $\Delta\sigma > 0$, is greater in magnitude than the increase in the offer for an equivalent risk decrease, $\varDelta\sigma < 0$.

Hypothesis 3.2. The decrease in the bid for a relative risk increase, $\varDelta\sigma > 0$, is greater in magnitude than the increase in the bid for an equivalent risk decrease, $\varDelta\sigma < 0$.

The next two hypotheses refer to absolute risk changes between assets ε and α and are based on the assumption that the average individual's risk preferences satisfy DARA.

Hypothesis 3.3. The bid for a risk increase is less than or equal to the offer for an equivalent risk decrease ($\sigma^{\alpha} > \sigma^{\varepsilon}$).

Hypothesis 3.4. The bid for a risk decrease is greater than or equal to the offer for an equivalent risk increase ($\sigma^{\alpha} < \sigma^{\varepsilon}$).

The following two hypotheses refer to relative risk changes in asset α, again based on the assumption that the average individual satisfies DARA.

Hypothesis 3.5. For $\varDelta\sigma > 0$, the bid decreases at a faster rate than the offer as $\varDelta\sigma$ increases.

Hypothesis 3.6. For $\varDelta\sigma < 0$, the bid increases at a slower rate than the offer as $|\varDelta\sigma|$ increases.

In the homogeneous public good tasks, WTA (WTP) may change relative to the private good tasks' WTA (WTP) if risk preferences change due to the decision affecting others. Changes in risk preferences between private and homogeneous public good tasks either decrease or increase the WTA-WTP disparity if individuals respectively behave more risk neutral or more risk averse in the homogeneous public good tasks. In the heterogeneous public goods tasks, social efficiency suggests that WTA and WTP increase in the other person's expected payout. Recall that $\mu_1^{\varepsilon} = \mu_2^{\varepsilon}$, thus fairness preferences predict a decrease in WTA and WTP for positive and negative differences in expected payouts between decision maker and other individual, $\mu_1^{\alpha} \neq \mu_2^{\alpha}$. Maximin preferences predict a negative WTA-WTP disparity for scenarios in which $\mu_1^{\alpha} > \mu_2^{\alpha}$.

3.3.3 Procedures

The data was collected in ten experimental sessions at the University of Tennessee, Knoxville in the Summer semester in 2013. A total of 168 undergraduate student subjects participated in the sessions. Each subject participated in one session only and participated in either a WTP or a WTA session. Each session consisted of 37 total decision rounds. Sessions lasted between one hour 30 minutes and one hour 45 minutes. On average, subjects earned \$19.40 (\$35.90) in the WTP (WTA) sessions. Differences in average earnings between the WTP and WTA sessions are the result of the inherent differences in property rights in the decision tasks.

To avoid potential experimental confounds, the experimental procedures followed the design choices of Plott and Zeiler (2005) closely. A monitor read the experimental instructions and explained how the computer program works to participants. The instructions described the optimal strategy in the BDM mechanism. Prior to the actual paid decision rounds, participants filled out a short paid quiz which tested them on their understanding of the buying (selling) mechanism. The monitor then individually checked participants' answers and thoroughly answered any questions that participants may have had to avoid any potential misconceptions. Further, the monitor went over the answers to the quiz questions with the whole group.

In each session, all subjects participated in three groups of paid decision tasks: (1) low stakes, individual buying (selling) tasks with certain payouts (six decision tasks), (2) high stakes, private buying (selling) tasks with uncertain payouts (eight decision tasks), (3) high stakes, group buying (selling) tasks with uncertain payouts (23 decision tasks). Subjects always participated in the low stakes, individual task block first. The second task block was either the individual high stakes block or the group high stakes block[7]. In between each task block, participants received a new set of instructions that described the next block. At the beginning of each block, participants first went trough one unpaid practice round so that they could familiarize themselves with the decision task. All six low stakes decision tasks were paid tasks and were resolved at the end of each task by the computer program. The low paid decision tasks served as training rounds that provided subjects with sufficient hands-on experience with the buying (selling) mechanism to eliminate any subject misconceptions.

In both high stakes task blocks, participants did not know how many decision tasks they would go through until the end of a decision task block. At the end of the session, the computer randomly selected at least one of the private good tasks and at least two of the public good tasks for actual payment (in at least one of these tasks, the participant was the decision maker, and in at least one of these tasks, the matched person was the decision maker). High stakes decisions were not resolved at the end of a decision round. Instead, the monitor informed participants about the random decision selection mechanism. Participants did not know how many high stakes decisions they

[7] I varied the order of the private and public good tasks within each session to control for potential order effects. Further, within each task block, I shuffled the order of specific tasks (treatments).

would make. The monitor pointed out that each decision task could be selected as one of the tasks that will determine subjects' earnings. In total, four (three) high stakes decision rounds were chosen for payment in each of the WTP (WTA) sessions. Karni and Safra (1987) demonstrate that the BDM elicitation mechanism may not always be incentive compatible; further, Holt (1986) points out that random decision selection (RDS) mechanism may not be incentive compatible under certain conditions (lotteries over lotteries argument). However, recent theoretical work (Azrieli et al. (2012a,b)) demonstrates that the RDS mechanism is incentive compatible if subjects' "ranking of gambles over outcomes respects the monotonicity axiom."

Participants made decisions on a fully-automated computer program in laboratory with networked computer workstations. In each buying (selling) decision, participants had to enter an bid (offer) amount into the computer. The computer program displayed initial asset and the alternative asset. The computer screen explained the stakes of an asset in words and also showed a graphical representation of the asset (Figure C.1, Figure C.2, and Figure C.3 show screenshots of the low stakes, individual high stakes, and group high stakes decision tasks in the WTP treatment. Figure C.7, Figure C.8, and Figure C.9 show screenshots of the low stakes, individual high stakes, and group high stakes decision tasks in the WTA treatment, respectively.). Figures C.4 through C.12 show the corresponding screenshots of the payout stages. I programmed all treatments in z-Tree (Fischbacher (2007)). Privacy screens on the monitors maintained private, anonymous decision making.

3.4 Experimental Results

3.4.1 Training Tasks

First, I present the results of the training tasks. Recall the optimal strategy in the BDM mechanism is to bid (offer) the difference in values between in initial and alternative asset. To test whether subjects bids (offers) converge to the optimal bid (offer), I regress the difference between bid (offer) and difference in payout, $b\,(o) - \Delta\mu$, on experiment covariates. Table 3.5 shows the regression results. The results suggest that offers exceed the difference in payouts. Bids are greater than the difference in payouts but less so than the offers. Importantly, in the last training round, bids are not significantly different from zero at the standard significance levels ($\widehat{b - \Delta}\mu = 0.03$, $t = 1.44$). Offers significantly exceed the difference in payouts in the last round ($\widehat{o - \Delta}\mu = 0.05$, $t = 2.75$). However, the amount that offers exceed the difference in payouts is only about \$0.05. This is similar to the findings of Plott and Zeiler (2005). I conclude that by the end of the training rounds, the majority of participants fully understood the optimal strategy in the BDM mechanism.

Table 3.5: Training Rounds Regression Estimates

Variable	$b\,(o) - \Delta\mu$	
Round	0.0008	(0.0043)
Last Round	-0.0150	(0.0247)
WTP·Last Round	0.0237	(0.0298)
WTP	-0.0490***	(0.0122)
Constant	0.0634***	(0.0155)
$N = 1,008,\ R^2 = 0.02,\ F = 4.27$***		

Note: The coefficient estimates are in dollars. Robust standard errors, clustered at the subject level, are in parantheses. Significance of coefficient estimates at the 1%, 5%, and 10% level is denoted by ***, **, and *.

3.4.2 Private and Public Good Tasks

To test the impact of welfare changes and changes in risk between the initial and the alternative asset on WTP (WTA), I conduct the following regression analysis which pools all observations from the private and public goods tasks:

$$
\begin{aligned}
(y_i - \Delta\mu) &= \boldsymbol{\beta'_0}\mathbf{1}_{\{\text{Treat.}\}} + \boldsymbol{\beta'_\oplus}\mathbf{1}_{\{\text{Treat.}\}}\Delta\sigma^{\oplus} + \boldsymbol{\beta'_\ominus}\mathbf{1}_{\{\text{Treat.}\}}\Delta\sigma^{\ominus} \\
&+ \boldsymbol{\beta'_1}\mathbf{1}_{\{\text{Treat.}\}}\delta\mu^{\oplus} + \boldsymbol{\beta'_2}\mathbf{1}_{\{\text{Treat.}\}}\delta\mu^{\ominus} + \qquad (3.4.1) \\
&+ \boldsymbol{\beta'_3}\mathbf{1}_{\{\text{Treat.}\}}\delta\sigma^{\oplus} + \boldsymbol{\beta'_4}\mathbf{1}_{\{\text{Treat.}\}}\delta\sigma^{\ominus} + \epsilon_i, \qquad (3.4.2)
\end{aligned}
$$

where y_i is either b_i or o_i. $\mathbf{1}_{\{\text{Treat.}\}}$ denotes a (6 x 1) indicator vector for the six distinct tasks: (1) private WTA, (2) private WTP, (3) public homogeneous WTA, (4) public homogeneous WTP, (5) public heterogeneous WTA, and (6) public heterogeneous WTP (e.g. $\mathbf{1}_{\{\text{Treat.}\}} = (0,0,1,0,0,0)'$ if the observation is a public heterogeneous

WTA task). Vector $\boldsymbol{\beta_0}$ denotes task-specific intercepts. Vectors $\boldsymbol{\beta}_\oplus$ and $\boldsymbol{\beta}_\ominus$ denote the coefficient vectors for relative risk increases and decreases, respectively. That is $\Delta\sigma^\oplus = \sigma^\alpha - \sigma^\varepsilon \geq 0$ and $\Delta\sigma^\ominus = \sigma^\alpha - \sigma^\varepsilon < 0$. Variables $\delta\mu^\oplus$, $\delta\mu^\ominus$, $\delta\sigma^\oplus$, and $\delta\sigma^\ominus$ are specific to the public good heterogeneous tasks. These variables capture social preferences with respect to relative changes in expected payouts and risk between the decision maker and the other person. Specifically, $\delta\mu^\oplus = \Delta\mu_1 - \Delta\mu_2 \geq 0$ and $\delta\mu^\ominus = \Delta\mu_1 - \Delta\mu_2 < 0$, where subscript 1 (2) denotes the decision maker's (other person's) expected difference in payout. The other two variables, $\delta\sigma^\oplus$ and $\delta\sigma^\ominus$, are constructed in a similar way with respect to relative risk changes between decision maker and other person.

Hypotheses 3.1 and 3.2 predict a negative relationship between WTA (WTP) and $\Delta\sigma^\oplus$ and a positive relationship between WTA (WTP) and $\Delta\sigma^\ominus$. Further, I expect the increase in WTA (WTP) as $\Delta\sigma^\ominus$ increases to be smaller than the decrease in WTA (WTP) as $\Delta\sigma^\oplus$ increases. According to hypothesis 3.5, the decrease in WTA should be less than the decrease in WTP as $\Delta\sigma^\oplus$ increases. Similarly, the increase in WTP should be greater than the increase in WTA as $\Delta\sigma^\ominus$ increases (3.6).

Social efficiency predicts that WTA and WTP decrease in $\delta\mu^\oplus$ and increase in $\delta\mu^\ominus$. Fairness predicts that both welfare measures decrease in $\delta\mu^\oplus$ and $\delta\mu^\ominus$. If individuals are concerned about the risk exposure of others, then there should be a negative (positive) relationship between the welfare measures and $\delta\sigma^\oplus$ ($\delta\sigma^\ominus$).

Table 3.6 reports the regression results with coefficient estimates for WTA and WTP across columns. The last column shows the differences in coefficient estimates between WTA and WTP. Note that an offer of \$12 (a bid of \$0) is equivalent to no sell (no buy) intentions. The regression only includes offers (bids) strictly less (greater) than \$12 (\$0). Table C.1 in the Appendix shows the regression estimates based on the full sample.[8] First, I discuss the coefficient estimates for the private good tasks. The intercept estimates for WTA and WTP are both significantly positive at the 1% significance level which indicates over-offering (over-bidding). The WTA intercept estimate exceeds the WTP intercept estimate by \$0.54, the difference is statistically significant at the 5% confidence level. The coefficient estimate on $\Delta\sigma^{\oplus}$ for WTA is positive, although not significantly different from zero. The coefficient estimate on $\Delta\sigma^{\oplus}$ for WTP is significantly negative (5% signifcance level) which indicates risk-aversion for risk increases. For each dollar that the standard deviation of asset α exceeds the standard deviation of asset ε, the bid decreases by \$0.21. The difference in coefficient estimates between WTA and WTP is positive but not significant (no support for hypothesis 3.5). The coefficient estimate on $\Delta\sigma^{\ominus}$ is significantly positive for both welfare measures (at the 1% significance level) which suggests risk aversion for risk reductions. For each dollar that the standard deviation of asset ε exceeds the standard deviation of asset α, the estimated offer (bid) decreases by \$0.72 (\$0.44). The difference in coefficient estimates between WTA and WTP is significantly positive at the 5% level (t-test) which supports hypothesis 3.6. Next, I test for differences in coefficient estimates for risk changes within a welfare measure.Within WTA, the coefficient estimate on $\Delta\mu^{\oplus}$ is significantly less in magnitude than the coefficient estimate on $\Delta\mu^{\ominus}$ (t-test with p-value <0.01) which contradicts hypothesis 3.1. Within WTP, the coefficient estimate on $\Delta\mu^{\oplus}$ is significantly less in magnitude than the coefficient estimate on $\Delta\mu^{\ominus}$ (t-test with p-value 0.06) which contradicts hypothesis 3.2. To test hypothesis 3.4, I look at the following scenario. Set $\Delta\mu = \$0$, $\Delta\sigma^{\ominus} = \$4$ (i.e. $\sigma^{\varepsilon} = \$4$, $\sigma^{\alpha} = \$0$ and $\mu^{\varepsilon} = \mu^{\alpha}$ which is the largest possible, pure risk reduction

[8] Estimates do not differ much qualitatively across the two samples. However, the difference in intercept coefficient estimates between WTA and WTP is greater in the full sample regression than in the restricted sample regression.

in WTP in the experiment); the estimated offer, $\hat{o}$ = \$3.88, is significantly greater than the estimated bid, $\hat{b}$ = \$2.23 (t-test with p-value <0.01). That is, even under a scenario that most likely yields a negative WTA-WTP disparity, the findings suggest a positive disparity. I do not find support for hypothesis 3.4. In general, the findings suggest that WTA is significantly greater than WTP for lotteries. I summarize the findings from the private good tasks as follows.

Result 3.1. The increase in WTA for a relative risk decrease in α, $\varDelta\sigma < 0$, is greater in magnitude than the decrease in WTA for an equivalent relative risk increase.

Result 3.2. The increase in WTP for a relative risk decrease in α, $\varDelta\sigma < 0$, is greater than the decrease in WTP for an equivalent relative risk decrease.

Result 3.3. WTA is greater than WTP for $\varDelta\sigma > 0$.

Result 3.4. WTA is greater than WTP for $\varDelta\sigma < 0$.

Result 3.5. For $\varDelta > 0$, WTP does not decrease at a different rate than WTA as $\varDelta\sigma$ increases.

Result 3.6. For $\varDelta < 0$, WTP increases at a slower rate than WTA as $|\varDelta\sigma|$ increases.

Note that the only difference between the private good and homogeneous public good tasks is that for the latter the decision affects another person who has the same proposed welfare and risk change as the decision maker. Comparing coefficient estimates between private good and homogeneous public good tasks yields the following findings. The difference in coefficient estimates on $\varDelta\sigma^{\oplus}$ is significantly positive at the 10% significance level. This is mainly driven by the significance of the WTP coefficient estimate - the WTA coefficient estimate is still not significantly different from zero. Analogous to the private good tasks, there is a significantly positive disparity between coefficient estimates on $\Delta\sigma^{\ominus}$. Together this is evidence in support of hypotheses 3.5 and 3.6. Within WTA, the coefficient on $\Delta\sigma^{\oplus}$ is significantly less in magnitude than the coefficient on $\Delta\sigma^{\ominus}$ (t-test with p-value of <0.01) which contradicts hypothesis 3.1. Within WTP, the coefficient on $\Delta\sigma^{\oplus}$ is less in magnitude than the coefficient on $\Delta\sigma^{\ominus}$, however the difference is not statistically significant (t-test with p-value of 0.65) (not hypothesis 3.2). Corresponding coefficient estimates on risk changes within a welfare measure do not differ significantly across private good and homogeneous public good tasks. This indicates that individuals' risk preferences when their decisions affect others are not different from their risk preferences when they make individual decisions.

The heterogeneous public good tasks differ from the homogeneous public good task in that the assets are different across decision maker and other person in terms relative expected payouts and relative risk changes. The intercept estimates for WTA and WTP are significantly greater than the corresponding estimates in the private good tasks (t-tests with p-values of <0.01 and 0.02). The WTA coefficient estimate on $\Delta\sigma^{\oplus}$ is significantly negative (1% signifcance level) which indicates risk-aversion. For each dollar that the standard deviation of asset α exceeds the standard deviation of asset ε, WTA decreases by \$0.42. The WTP coefficient on $\Delta\sigma^{\oplus}$ is negative but not significant. The difference in coefficient estimates between WTA and WTP is significantly negative at the 5% significance level (contradicts hypothesis 3.5). The finding in terms of the WTA coefficient estimate on $\Delta\sigma^{\ominus}$ is smaller in magnitude than in the public good homogeneous task (t-tests with p-values of <0.01). The WTP coefficient on $\Delta\sigma^{\ominus}$ is smaller in magnitude than in the public good homogeneous task yet the difference is not signifcant (t-test with p-value of 0.30). The WTA coefficient is significantly greater than the WTP coefficient estimate (t-test with p-value of 0.05) which supports hypothesis 3.6. Within WTA, the coefficient on $\Delta\sigma^{\oplus}$ is not significantly different in magnitude from the coefficient on $\Delta\sigma^{\ominus}$ (t-test with p-value of 0.80). Within WTP, the absolute value of the coefficient on $\Delta\sigma^{\oplus}$ is not significantly different in magitude than from the coefficient on $\Delta\sigma^{\ominus}$ (t-test with p-value of 0.34).

In terms of social preferences, the coefficient estimates on $\delta\mu^{\oplus}$ are significantly negative (1% significance level) which suggest that subjects display altruism when their own gain in expected payout exceeds the other person's expected gain. For each dollar that the decision maker's expected payout gain exceeds the other person's expected payout gain, the decision maker decreases her offer (bid) by a \$0.42 (\$0.41). The coefficient estimate on $\delta\mu^{\ominus}$ is significantly positive at the 1% significance level for both welfare measures. For each dollar that the decision maker's expected payout gain falls short of the other person's expected payout gain, she increases her offer (bid) by a \$0.23 (\$0.17). Nonetheless, there is no significant disparity across welfare measures in terms of altruism. Fairness predicts that the coefficients estimates on $\delta\mu^{\ominus}$ and $\delta\mu^{\oplus}$ are negative for both WTA and WTP. Clearly, this is not the case here. However, there is a significant asymmetry between coefficient estimates on $\delta\mu^{\oplus}$ and $\delta\mu^{\ominus}$ within both welfare measures. The estimates on $\delta\mu^{\oplus}$ are significantly greater in magnitude than the coefficient estimates on $\delta\mu^{\ominus}$ (t-tests with p-values of 0.03 and <0.01). This asymmetry is in line with the predictions of quasi-maximin preferences.

Result 3.7. WTA and WTP decreases (increases) when the individual's expected payout gain is more (less) than the other person's expected payout gain.

The coefficient estimates on changes in relative risk indicate some evidence of the presence of social preferences with respect to changes in risk. The WTA coefficient estimate on $\delta\sigma^{\oplus}$ is significantly positive at the 1% significance level. For each dollar that the other person's relative risk is less than the decision maker's relative risk, the individual is offering an additional $0.34. The corresponding WTP coefficient estimate is positive but not significant. The difference in coefficient estimates is significantly positive across welfare measures (t-test with p-value of 0.05).The coefficient estimates on $\delta\sigma^{\ominus}$, on the other hand, are not significantly different from zero.

Result 3.8. WTA increases when the individual's proposed relative risk change is greater than the other person's proposed risk improvement.

Table 3.6: Public Good Regression Estimates

		Variable	$(o - \Delta\mu)$		$(b - \Delta\mu)$		Difference	
Homogeneous	Private	Intercept	1.00***	(0.19)	0.46***	(0.17)	0.54**	(0.26)
		$\Delta\sigma^{\oplus}$	0.02	(0.13)	-0.21**	(0.09)	0.23	(0.16)
		$\Delta\sigma^{\ominus}$	0.72***	(0.10)	0.44***	(0.07)	0.28**	(0.12)
	Public	Intercept	1.20***	(0.24)	0.52**	(0.21)	0.68**	(0.32)
		$\Delta\sigma^{\oplus}$	-0.04	(0.12)	-0.30***	(0.09)	0.26*	(0.15)
		$\Delta\sigma^{\ominus}$	0.71***	(0.09)	0.36***	(0.08)	0.35***	(0.12)
Heterogeneous	Public	Intercept	2.11***	(0.25)	1.04***	(0.27)	1.07***	(0.37)
		$\Delta\sigma^{\oplus}$	-0.42***	(0.09)	-0.12	(0.08)	-0.30**	(0.13)
		$\Delta\sigma^{\ominus}$	0.45***	(0.07)	0.25***	(0.08)	0.21**	(0.10)
		$\delta\mu^{\oplus}$	-0.42***	(0.05)	-0.41***	(0.04)	-0.02	(0.07)
		$\delta\mu^{\ominus}$	0.23***	(0.05)	0.17***	(0.04)	0.06	(0.06)
		$\delta\sigma^{\oplus}$	0.34***	(0.08)	0.12	(0.08)	0.23*	(0.12)
		$\delta\sigma^{\ominus}$	0.02	(0.06)	0.09	(0.06)	-0.07	(0.09)
$N = 4,747,\ R^2 = 0.37,\ F = 49.10$***								

Note: Coefficients estimates based on pooled model. $N = 4,747$ ($0 bids and $12 offers excluded from estimation). WTA and WTP specific parameters are shown across columns for comparison. All coefficient estimates are in dollars. Robust standard errors, clustered at the subject level (168 subjects), are in parantheses. Significance of statistic at the 1%, 5%, and 10% is denoted by ***, **, and *, respectively.

3.5 Discussion and Conclusion

In this article, I studied the impact of uncertainty on the disparity between WTA and WTP measures for private and public good lotteries. This study differs from previous work in that it provides a rigorous experimental investigation of the effect of uncertainty on the disparity. Importantly, the decision tasks includes scenarios where a proposed policy change decreases (increases) risk in WTP (WTA). Previous work has not addressed uncertainty in a public good setting although public policies that target risk reductions in the context of health and the environment inherently involve uncertainty.

Using incentive compatible elicitation, I showed that the disparity persists for both private good and public good lotteries in the lab. I also found a large inherent disparity that cannot be explained by the experimental parameters in the econometric specification. The econometric specification assumes that the relationship between WTA (WTP) and changes in risk is linear. However, risk aversion suggests non-linearities. An extended model that allows for non-linearities may be able to explain some of the disparity. Further, I can use the private good tasks to construct a measure of risk aversion for each subject which allows for a more rigorous analysis of the public good tasks. Nonetheless, the observed disparity indicates the presence of an endowment effect. Previous empirical work showed that market experience plays an important role in minimizing the endowment effect (List, 2003). In terms of the experiment, it is possible that subjects in the WTA condition were not as experienced with the selling task as subjects were with the buying task in the WTP condition. One important extension of the analysis is to explore whether subjects' demographic characteristics and personality traits can explain part of the WTA-WTP disparity. I collected this information in post-experiment surveys.

The empirical results from the heterogeneous public good tasks provide evidence of social efficiency preferences. Participants care about changes in others' expected payouts. In terms of changes in expected payout relative to others, I found that participants are more altruistic when they are relatively worse off compared to when they are better off. This finding confirms previous results in the absence of unccertainty (Messer et al., 2010). Importantly, the experiments also showed that individuals have social preferences with regards to changes in risk. I observed social risk preferences in the context of relative risk increases in WTA. As mentioned before, the private good tasks can be used to investigate how individuals' private risk preferences affect their social risk preferences in the public good tasks.

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Appendices

Appendix A

The Impact of Forward Trading on Tacit Collusion

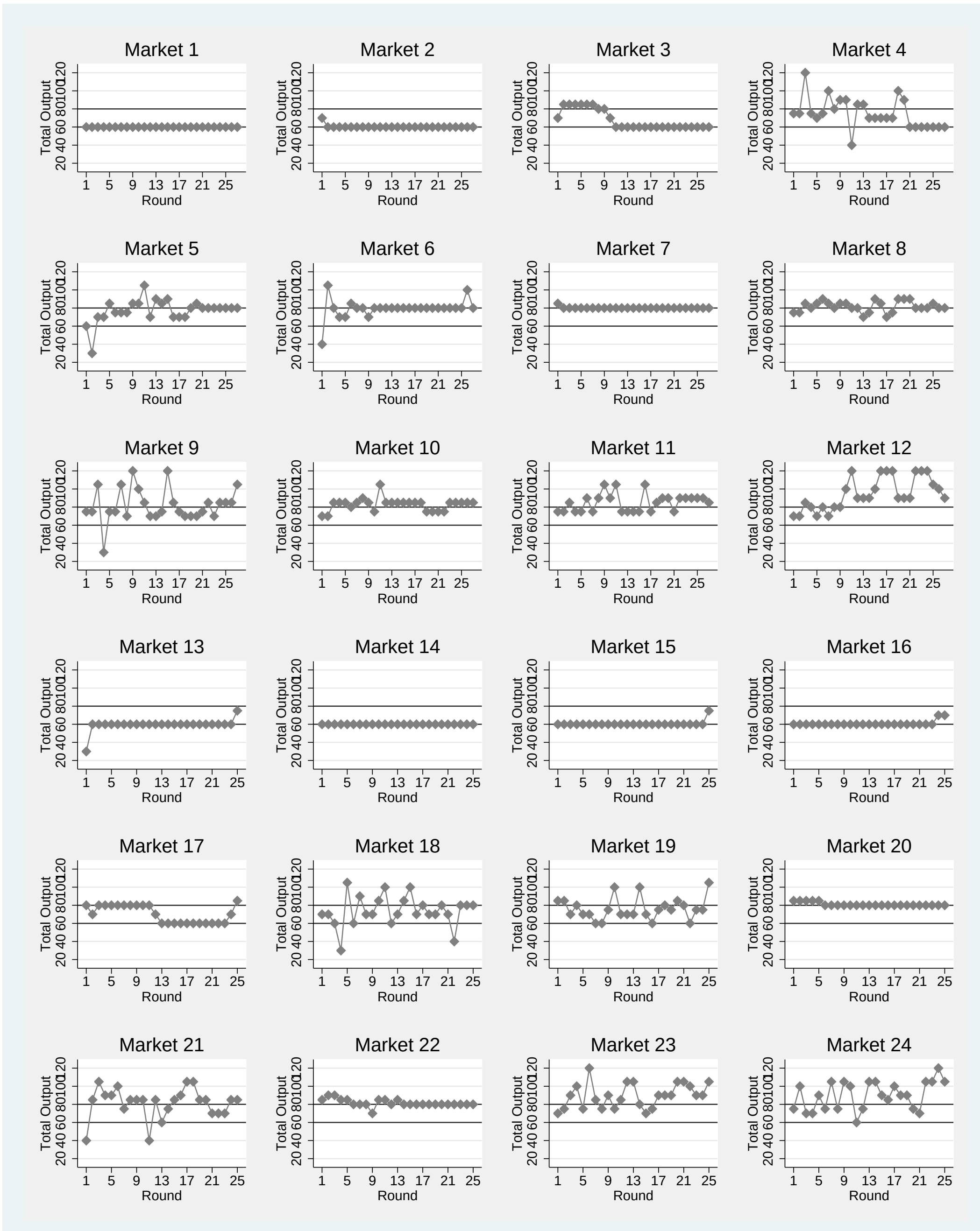
A.1 Termination Rule

I count each market as a single observation to account for possible interdependence of observations within a single market. The average chosen strategy in the known-end C2 treatment is lower (1.74) than the average strategy in the unknown-end treatment (2.08). This difference is not significant (Wilcoxon rank sum test, $z = -1.04$, $p = 0.30$). There is no observable difference in average chosen strategies between the two different termination rules in the C3 treatment (Wilcoxon rank sum test, $z = -0.17$, $p = 0.87$).

Next, I test for changes in subjects' decisions at the end of the game by comparing chosen strategies in a short period at the end of the game to chosen strategies in 10 prior rounds. Specifically, in the known- (unknown-) end termination treatments, I compare the average chosen strategies in a market in rounds 14-23 (16-25) to the average strategies in rounds 24-25 (26-27), respectively. Counting each market as a single observation, I find no statistically significant difference in the distribution of chosen strategies between the unknown-end C2 treatment, the known- and unknown-end C3 treatments, and the FS2 treatment (matched-pairs Wilcoxon signed-rank test with p-values ranging from 0.21 - 0.97). Although not significant, note that average chosen strategies are lower in the last two rounds in both C3 termination rule treatments. In the known-end C2 treatment, however, the average chosen strategy is significantly greater in the last two rounds compared to the 10 rounds prior (matched pairs Wilcoxon, $z = -2.86$, $p = 0.004$). Comparing average chosen strategies in rounds 15-24 (17-26) to average strategies in the final round yields similar results.

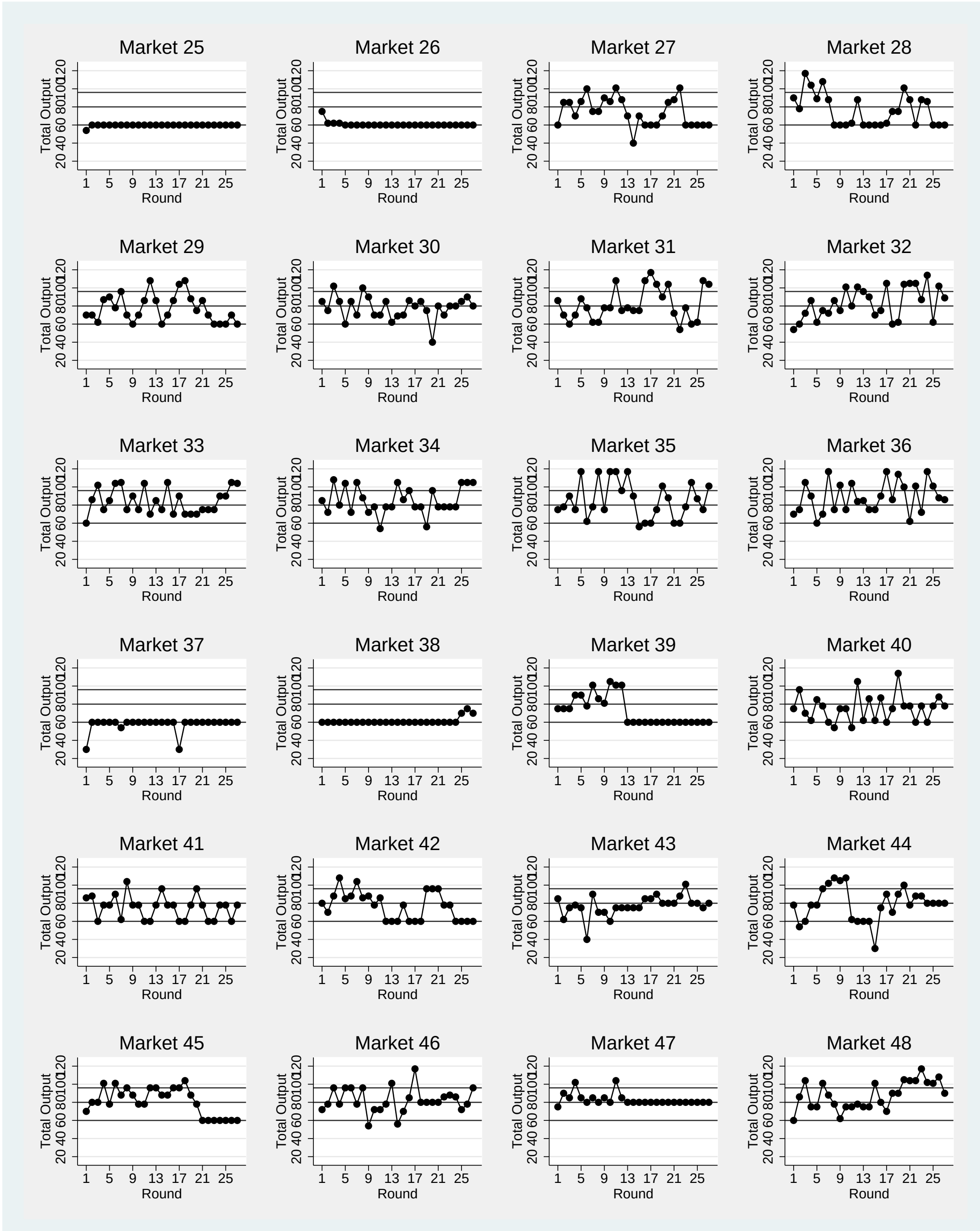
Average chosen strategies in the known-end C2 treatment are lower than average strategies in the unknown-end C2 treatment. I test whether average chosen strategies in round 24 of the known-end C2 treatment differ from average strategies in rounds 18-27 in the unknown-end C2 treatment: I find no significant difference in average chosen strategies (Wilcoxon rank sum test, $z = -0.56$, $p = 0.58$). Based on these results, I drop the final round from the known-end C2 data and pool unknown- and known-end termination rule sessions.

A.2 Figures



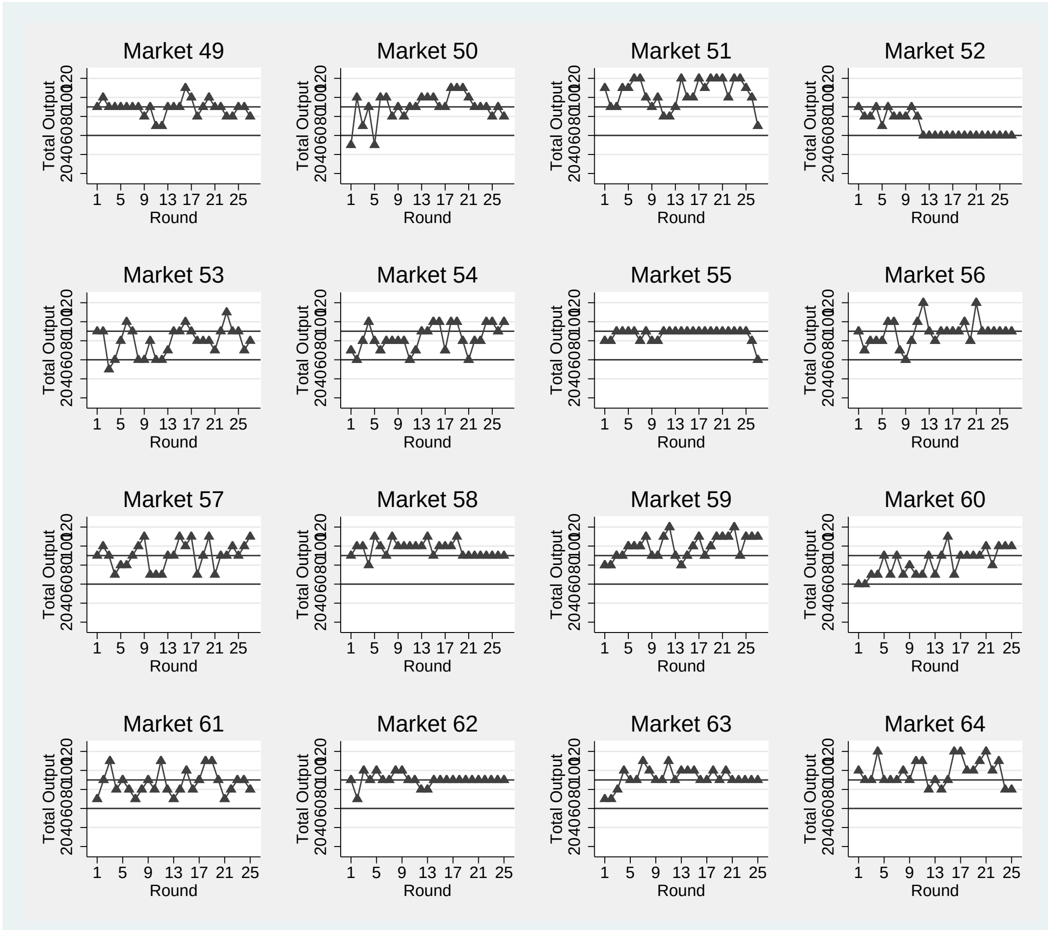
Note: Markets 1-12 (13-24) in unknown- (known-) end termination rule treatment.

Figure A.1: Total Output per Round, C2 Treatment, All Markets



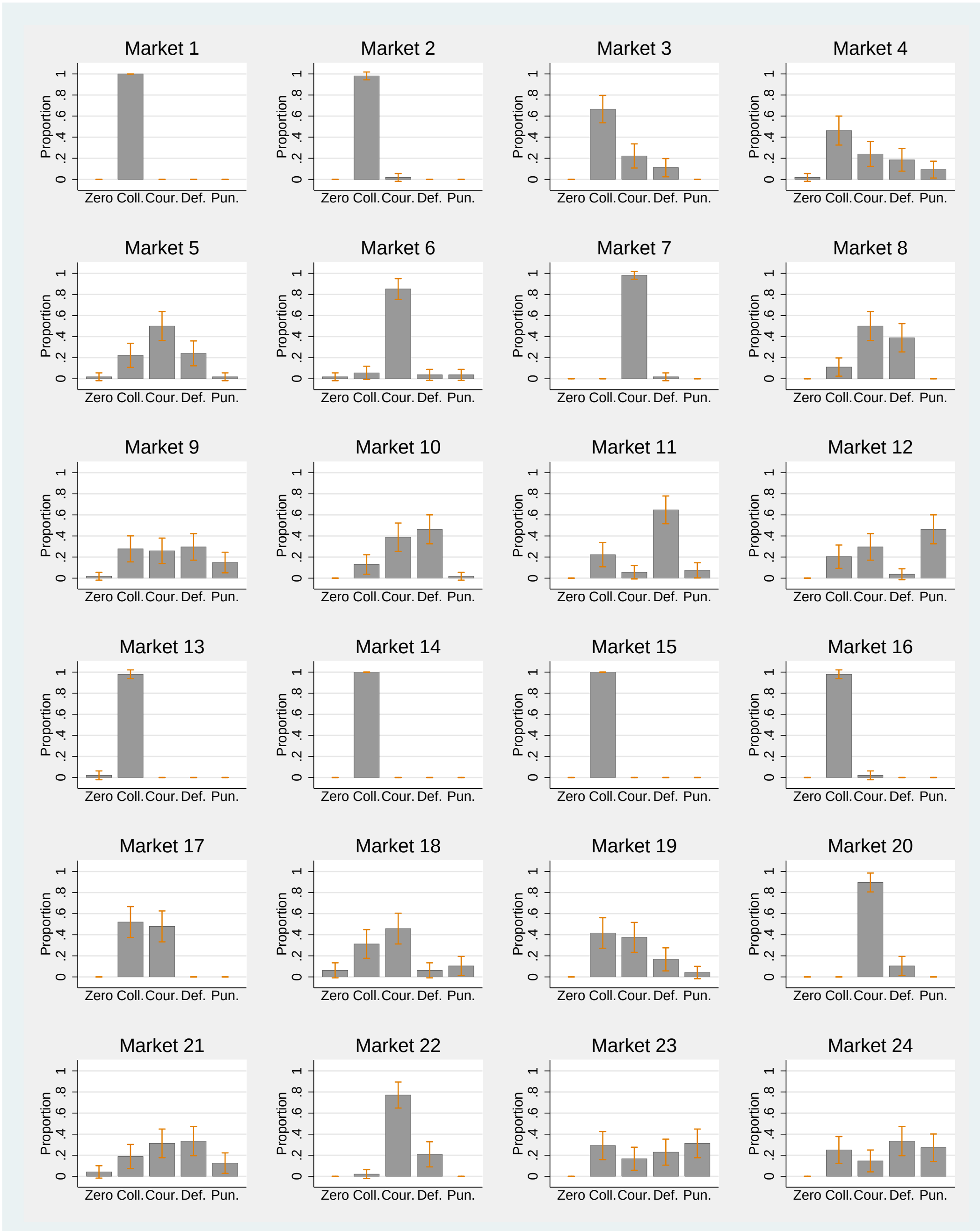
Note: Markets 25-36 (37-48) in Summer (Spring) session.

Figure A.2: Total Output per Round, FS2 Treatment, All Markets



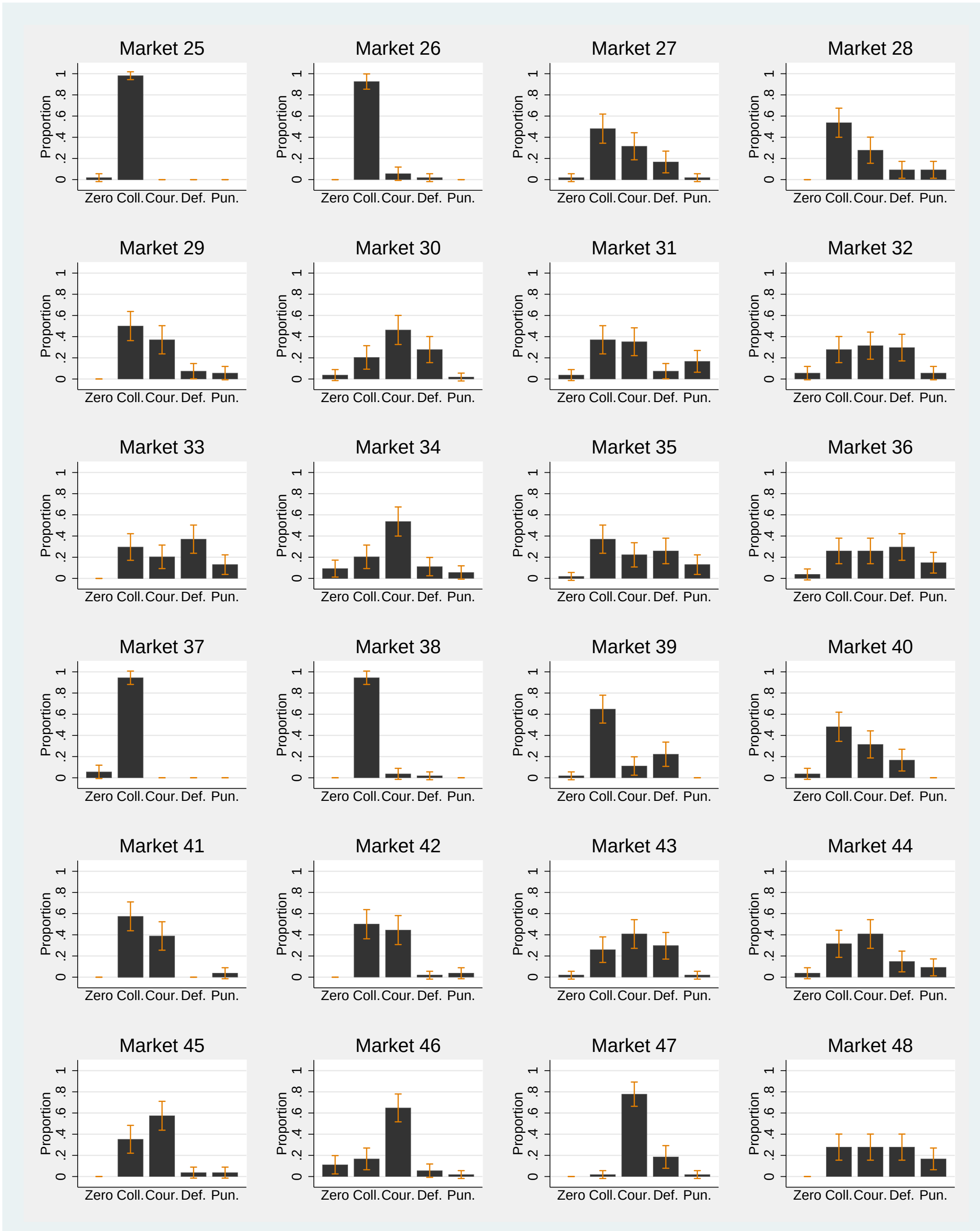
Note: Markets 49-59 (60-64) in unknown- (known-) end termination rule treatment.

Figure A.3: Total Output per Round, C3 Treatment, All Markets



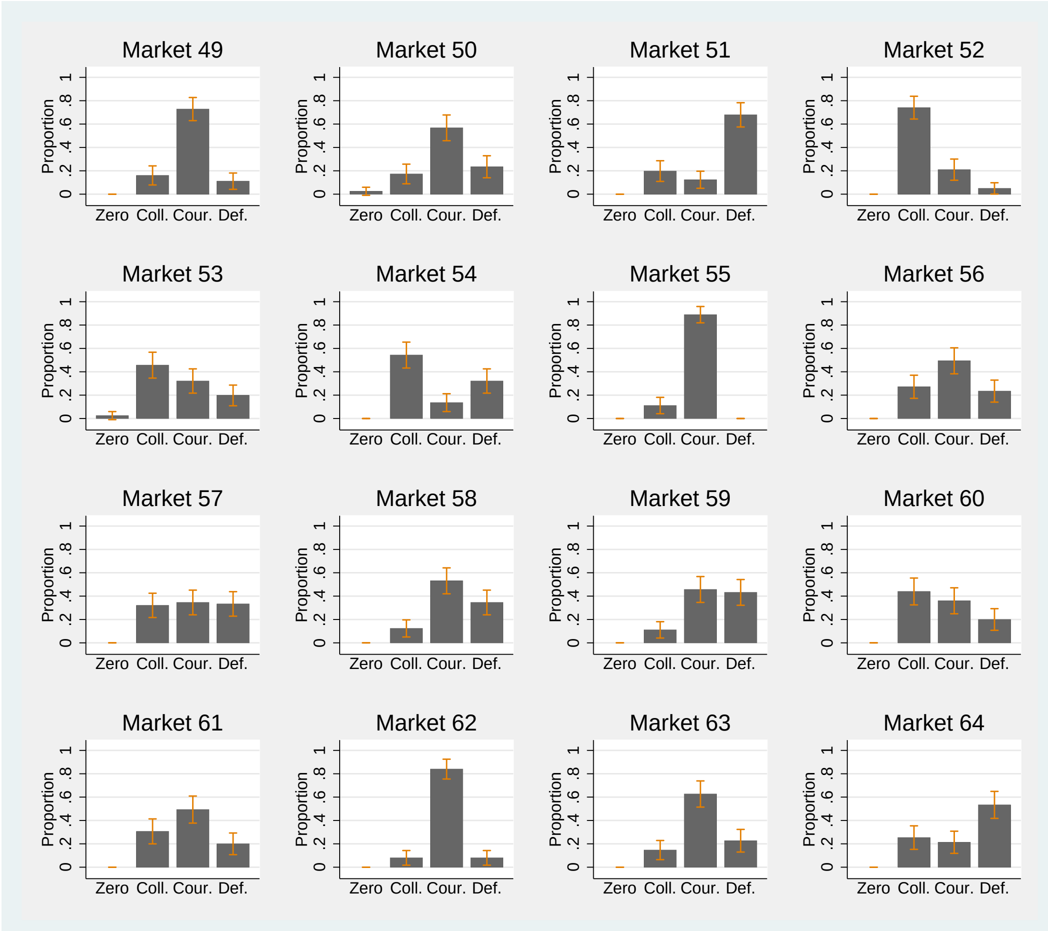
Note: Markets 1-12 (13-24) in unknown- (known-) end termination rule treatment.

Figure A.4: Proportion of Strategies with 95% Conf. Int., C2 Treatment, All Markets



Note: Markets 25-36 (37-48) in Summer (Spring) session.

Figure A.5: Proportion of Strategies with 95% Conf. Int., FS2 Treatment, All Markets



Note: Markets 49-59 (60-64) in unknown- (known-) end termination rule treatment.

Figure A.6: Proportion of Strategies with 95% Conf. Int., C3 Treatment, All Markets

A.3 Instructions

A.3.1 Single Phase Treatment

Brackets, [], denote differences between two-firm and three-firm instructions.

You are about to participate in an experiment in economic decision making. If you follow the instructions carefully, you can earn a considerable amount of money. At the end of today's session, you will be paid your earnings in private and in cash.

Overview

The experiment will last several decision rounds. You will not know the number of rounds until the end of the experiment. At the beginning of the session, you will be randomly and anonymously matched with one [two] other person[s]. The one [two] other person[s] with whom you will be matched will be the same in every round, but you will not learn the identity of the other person[s]. The decisions that you and the other [two] person[s] make will determine the dollar earnings for each of you.

In this session, you are a quantity-setting seller of a hypothetical good. You will earn profits by selling units of the good. At the beginning of each round, you will be asked how many units of the good you want to sell in that round. You make a decision by selecting a number from a list on your computer. The possible choices are 0, 30, 40, 45, or 60 [0, 20, 30, or 40] units. At the same time that you are submitting how many units you want to sell, the other [two] seller[s] in your 2[3]-seller market will also submit how many units he/she [they] wants to sell. None of you will be able to see the decisions of the other [two] seller[s] in your market until both [all three] of you have submitted your decisions. Note that once submitted, all decisions are final and cannot be changed. At the end of each round, you will see how many units you sold, how many units the other seller[s] in your 2[3]-seller market sold, how many total units were sold, the price for that round, and your earnings for that round. Earnings are denoted in tokens and each unit has a cost of 0 (zero) tokens to you.

Price Calculation

Buyers are automated by the computer program. The market price at the end of a round will be determined by the units sold by both [all three] sellers in your 2[3]-seller market in a round. At the end of a round, the computer will calculate the market price (in tokens) as follows:

$$\text{Price} = 120 - \text{Total Units Sold Stage}$$

In general, the higher the number of total units sold the lower the price and vice versa. If the total amount of units sold across both stages is greater than or equal to 120, the price will be zero. Hint: The number of total sales in your 2[3]-seller market can never be greater than 120 in any round.

Earnings

You will earn profits by selling units. The profit for any unit sold is the selling price in that round. Your total earnings (in tokens) in a round will be calculated as follows:

$$\text{Round Earnings} = \text{Price} \cdot \text{Your Total Units Sold}$$

Your total earnings in this part of the experiment will be your total earnings from all rounds. At the end of the first experiment, tokens will be converted into U.S. dollars at a rate of 1,800 tokens per U.S. dollar.

The following table shows your possible earnings in each round based on the sales choices that you and the other seller in your 2[3]-seller market make:

Other Seller's Total Units Sold

Your Units Sold	**0**	**30**	**40**	**45**	**60**
0	0	0	0	0	0
30	2,700	1,800	1,500	1,350	900
40	3,200	2,000	1,600	1,400	800
45	3,375	2,025	1,575	1,350	675
60	3,600	1,800	1,200	900	0

Other Two Sellers' Total Units Sold

Your Units Sold	**0**	**20**	**30**	**40**	**50**	**60**	**70**	**80**
0	0	0	0	0	0	0	0	0
20	2,000	1,600	1,400	1,200	1,000	800	600	400
30	2,700	2,100	1,800	1,500	1,200	900	600	300
40	3,200	2,400	2,000	1,600	1,200	800	400	0

Before making any final decisions, you will be asked to answer 4 (four) practice questions to verify that you understand how your earnings are determined.

Computer Program

At the top of your screen, you will see a payout table similar to the table above (gray frame). In the middle of your screen, you will see the actual decision panel (orange frame). You will make a decision by selecting how many units you want to sell from the list. Once you click the "Submit" button, your sales decision cannot be changed and will be final. At the end of each round, the computer will display your sales, the other [two] seller's sales, the total sales in your 3-seller market, the price, and your profit for that round. The computer will keep track of your sales, the other seller's sales, the price, and your earnings in each round. This information will be displayed in a table at the bottom of your computer screen (gray frame). The computer will also update your total earnings which will be displayed at the top of your screen (in tokens).

Summary

- The experiment will last several decision rounds. You will not know the number of rounds until the end of the experiment.
- You will be randomly matched with one [two] other seller[s]. The one [two] other seller[s] with whom you will be matched will be the same in every round!
- You will earn profits by selling units. The profit for any unit sold is the selling price in that round.
- The price that you will receive for each unit you sell in a round is calculated as follows: Price = 120 − Total Units Sold
- Your earnings will be the sum of your earnings from all rounds.

If you have a question at any point during the experiment, please raise your hand! One of the monitors will come to your station and answer it in private.

A.3.2 Two-Phase Treatment

You are about to participate in an experiment in economic decision making. If you follow the instructions carefully, you can earn a considerable amount of money. At the end of today's session, you will be paid your earnings in private and in cash.

Overview

The experiment will last several decision rounds. You will not know the number of rounds until the end of the experiment. At the beginning of the session, you will be randomly and anonymously matched with one other person. The one other person with whom you will be matched will be the same in every round, but you will not learn the identity of the other person. The decisions that you and the other person make will determine the dollar earnings for each of you.

In this session, you are a quantity-setting seller of a hypothetical good. You will earn profits by selling units of the good. Each round consists of two stages (A and B). At the beginning of each stage, you will be asked how many units of the good you want to sell in that stage. You make a decision by selecting a number from a list on your computer. (In stage A, the possible choices are 0 and 24 units. In stage B, you will have five choices which depend on the decisions in stage A.) At the same time that you are submitting how many units you want to sell, the other seller in your 2-seller market will also submit how many units he/she wants to sell. None of you will be able to see the decisions of the other seller in your market until both of you have submitted your decisions. Note that once submitted, all decisions are final and cannot be changed.

At the end of stage A, you will see how many units you sold, how many units the other seller in your 2-seller market sold and how many units were sold in total in Stage A. At the end of stage B, you will see how many units you sold, how many units the other seller in your 2-seller market sold, the price for that round, and your earnings for that round. Earnings are denoted in tokens and each unit has a cost of 0 (zero) tokens to you.

Price Calculation

Buyers are automated by the computer program. The market price at the end of a round will be determined by the units sold by both sellers in your 2-seller market in a round in stage A and stage B combined. At the end of a round, the computer will calculate the market price (in tokens) as follows:

$$\text{Price} = 120 - \text{Total Units Sold Stage A} - \text{Total Units Sold Stage B}$$

In general, the higher the number of total units sold the lower the price and vice versa. If the total amount of units sold across both stages is greater than or equal to 120, the price will be zero. Hint: The number of total sales in your 2-seller market can never be greater than 120 in any round.

Earnings

You will earn profits by selling units. The profit for any unit sold is the selling price in that round. Your total earnings (in tokens) in a round will be calculated as follows:

$$\text{Round Earnings} = \text{Price} \cdot \text{Your Total Units Sold}$$

Note: You will receive the same price for any unit sold in stage A and/or stage B. Your total earnings in the experiment will be your total earnings from all rounds. At the end of the first experiment, tokens will be converted into U.S. dollars at a rate of 1,800 tokens per U.S. dollar.

Attached is a table that shows your possible earnings in each round based on the sales choices that you and the other seller in your 2-seller market make in both stages. Before making any final decisions, you will be asked to answer 4 (four) practice questions to verify that you understand how your earnings are determined.

Computer Program

At the top of your screen, you will see a payoff table similar to the table above (gray frame). In the middle of your screen, you will see the actual decision panel (orange frame). You will make a decision by selecting how many units you want to sell from the list. Once you click the "Submit" button, your sales decision cannot be changed and will be final.

At the end of each round, the computer will display your sales, the other seller's sales, the total sales in your 2-seller market, the price, and your earnings for that round. The computer will keep track of your sales, the price, and your earnings in each round. This information will be displayed in a table at the bottom of your computer screen (gray frame). The computer will also update your total earnings which will be displayed at the top of your screen (in tokens).

Summary

- The experiment will last several decision rounds. You will not know the number of rounds until the end of the experiment.
- You will be randomly matched with one other seller. The one other seller with whom you will be matched will be the same in every round!
- You will earn profits by selling units in either stage A, or stage B, or both stages. The profit for any unit sold is the selling price in that round.
- The price that you will receive for each unit you sell in a round is calculated as follows: Price = 120 − Total Units Sold Stage A − Total Units Sold Stage B
- Your earnings will be the sum of your earnings from all rounds.

If you have a question at any point during the experiment, please raise your hand! One of the monitors will come to your station and answer it in private.

My Sales A	**Other Seller's Sales A**	**My Sales B**	**Other Seller's Sales B**	**Total Sales**	**Price**	**My Profit**	**Other Seller's Profit**
0	0	0	0	0	120	0	0
0	0	0	30	30	90	0	2,700
0	0	0	40	40	80	0	3,200
0	0	0	45	45	75	0	3,375
0	0	0	60	60	60	0	3,600
0	0	30	0	30	90	2,700	0
0	0	30	30	60	60	1,800	1,800
0	0	30	40	70	50	1,500	2000
0	0	30	45	75	45	1,350	2025
0	0	30	60	90	30	900	1800
0	0	40	0	40	80	3,200	0
0	0	40	30	70	50	2,000	1,500
0	0	40	40	80	40	1,600	1,600
0	0	40	45	85	35	1,400	1,575
0	0	40	60	100	20	800	1,200
0	0	45	0	45	75	3,375	0
0	0	45	30	75	45	2,025	1,350
0	0	45	40	85	35	1,575	1,400
0	0	45	45	90	30	1,350	1,350
0	0	45	60	105	15	675	900
0	0	60	0	60	60	3,600	0
0	0	60	30	90	30	1,800	900
0	0	60	40	100	20	1,200	800
0	0	60	45	105	15	900	675
0	0	60	60	120	0	0	0

Table continued on next page.

My Sales A	Other Seller's Sales A	My Sales B	Other Seller's Sales B	Total Sales	Price	My Profit	Other Seller's Profit
0	24	0	0	24	96	0	2,304
0	24	0	6	30	90	0	2,700
0	24	0	32	56	64	0	3,584
0	24	0	33	57	63	0	3,591
0	24	0	48	72	48	0	3,456
0	24	30	0	54	66	1,980	1,584
0	24	30	6	60	60	1,800	1,800
0	24	30	32	86	34	1,020	1,904
0	24	30	33	87	33	990	1,881
0	24	30	48	102	18	540	1,296
0	24	32	0	56	64	2,048	1,536
0	24	32	6	62	58	1,856	1,740
0	24	32	32	88	32	1,024	1,792
0	24	32	33	89	31	992	1,767
0	24	32	48	104	16	512	1,152
0	24	45	0	69	51	2,295	1,224
0	24	45	6	75	45	2,025	1,350
0	24	45	32	101	19	855	1,064
0	24	45	33	102	18	810	1,026
0	24	45	48	117	3	135	216
0	24	48	0	72	48	2,304	1,152
0	24	48	6	78	42	2,016	1,260
0	24	48	32	104	16	768	896
0	24	48	33	105	15	720	855
0	24	48	48	120	0	0	0

Table continued on next page.

My Sales A	Other Seller's Sales A	My Sales B	Other Seller's Sales B	Total Sales	Price	My Profit	Other Seller's Profit
24	0	0	0	24	96	2,304	0
24	0	0	30	54	66	1,584	1,980
24	0	0	32	56	64	1,536	2,048
24	0	0	45	69	51	1,224	2,295
24	0	0	48	72	48	1,152	2,304
24	0	6	0	30	90	2,700	0
24	0	6	30	60	60	1,800	1,800
24	0	6	32	62	58	1,740	1,856
24	0	6	45	75	45	1,350	2,025
24	0	6	48	78	42	1,260	2,016
24	0	32	0	56	64	3,584	0
24	0	32	30	86	34	1,904	1,020
24	0	32	32	88	32	1,792	1,024
24	0	32	45	101	19	1,064	855
24	0	32	48	104	16	896	768
24	0	33	0	57	63	3,591	0
24	0	33	30	87	33	1,881	990
24	0	33	32	89	31	1,767	992
24	0	33	45	102	18	1,026	810
24	0	33	48	105	15	855	720
24	0	48	0	72	48	3,456	0
24	0	48	30	102	18	1,296	540
24	0	48	32	104	16	1,152	512
24	0	48	45	117	3	216	135
24	0	48	48	120	0	0	0

Table continued on next page.

My Sales A	Other Seller's Sales A	My Sales B	Other Seller's Sales B	Total Sales	Price	My Profit	Other Seller's Profit
24	24	0	0	48	72	1,728	1,728
24	24	6	0	54	66	1,980	1,584
24	24	24	0	72	48	2,304	1,152
24	24	33	0	81	39	2,223	936
24	24	36	0	84	36	2,160	864
24	24	0	6	54	66	1,584	1,980
24	24	6	6	60	60	1,800	1,800
24	24	24	6	78	42	2,016	1,260
24	24	33	6	87	33	1,881	990
24	24	36	6	90	30	1,800	900
24	24	0	24	72	48	1,152	2,304
24	24	6	24	78	42	1,260	2,016
24	24	24	24	96	24	1,152	1,152
24	24	33	24	105	15	855	720
24	24	36	24	108	12	720	576
24	24	0	33	81	39	936	2,223
24	24	6	33	87	33	990	1,881
24	24	24	33	105	15	720	855
24	24	33	33	114	6	342	342
24	24	36	33	117	3	180	171
24	24	0	36	84	36	864	2,160
24	24	6	36	90	30	900	1,800
24	24	24	36	108	12	576	720
24	24	33	36	117	3	171	180
24	24	36	36	120	0	0	0

A.4 Screen Shots

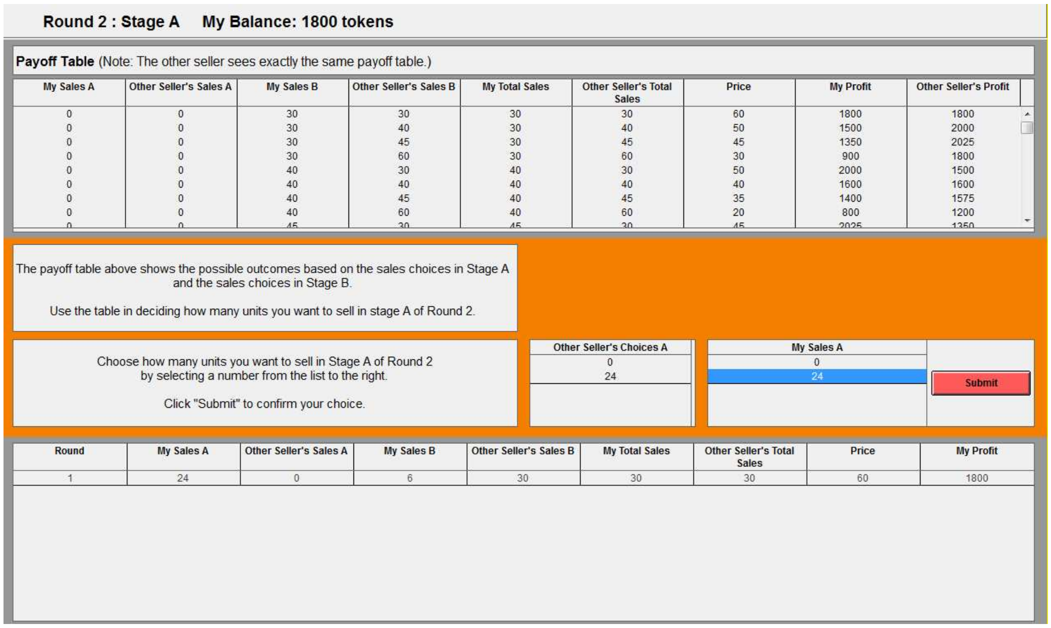


Figure A.7: Decision Stage A, FS2 Treatment

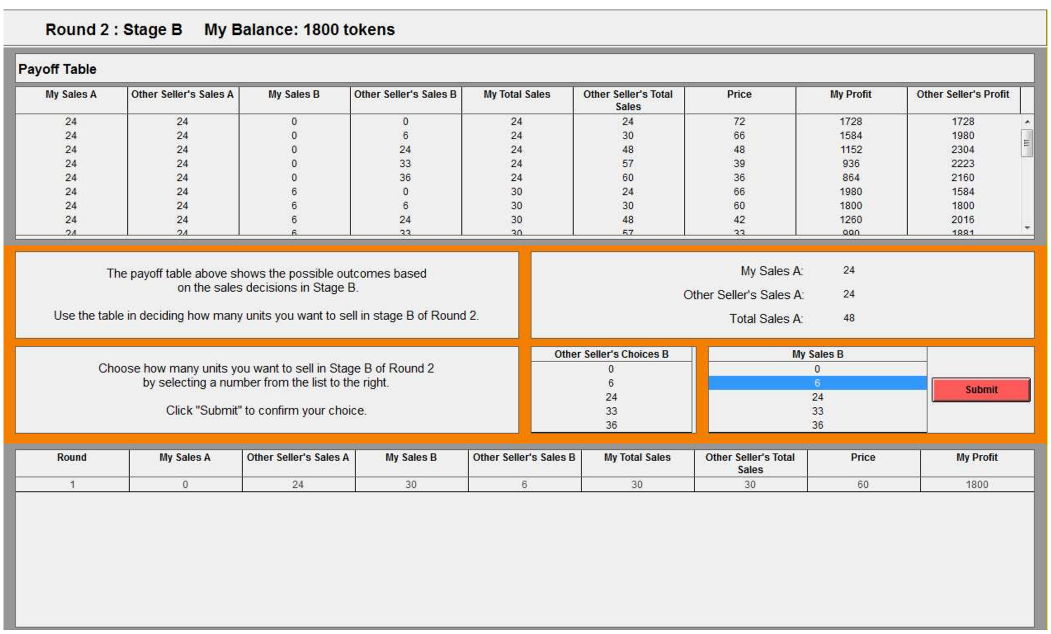


Figure A.8: Decision Stage B, FS2 Treatment

Appendix B

Environmental Quality and Investor Behavior

B.1 Robustness Checks

B.1.1 S&P 500 Market-Portfolio Estimates

The following tables show the regression estimates based on the S&P 500 index as market-portfolio. The results are very similar to those based on the CRSP value-weighted index.

Table B.1: Iceland Volcano Estimates, 6-Day Event Window

Variable	(1) CAR_{6d}		(2) CAR_{6d}		Mean $\widehat{CAR}_{6d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	0.01	(0.01)				
Banks and Insurance	-		0.06*	(0.03)	0.94*	(0.51)
Basic Materials	-		0.11**	(0.04)	-2.81***	(0.75)
Consumer Products, Cars	-		-0.05*	(0.02)	1.87***	(0.49)
Financial Services	-		0.12**	(0.05)	-1.44**	(0.64)
Food & Beverage	-		-0.06	(0.07)	-0.57*	(0.34)
General Industrials	-		-0.03%	(0.03)	2.12***	(0.65)
Health Care	-		-0.06**	(0.03)	-0.74	(0.50)
Industrial Goods	-		0.03**	(0.02)	0.52	(0.40)
Media, Travel, Leisure	-		0.02	(0.04)	-0.46	(0.80)
Oil and Gas	-		0.04	(0.07)	0.45	(0.48)
Pharmaceuticals	-		0.06	(0.07)	-3.50***	(0.78)
Retail	-		0.02	(0.05)	0.35	(0.43)
Technology	-		-0.03	(0.02)	0.44	(0.44)
Transportation, Aerospace	-		0.06**	(0.03)	1.88***	(0.61)
Utilities	-		-0.01	(0.02)	<0.01	(0.33)
Firm-Level						
ln (Mkt. Value)	-0.11	(0.20)	-0.10	(0.20)	-	
ln (Sales)	0.01	(0.21)	0.06	(0.21)	-	
EpS	-0.09	(0.07)	-0.06	(0.06)	-	
N	484		484			
R^2	0.17		0.22			
F	4.39***		3.79***			

Notes: Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively. The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: ACS, BRK3, BDK, BKC, DTV, ETN, FAF, FO, GENZ, JAVA, KFT, LSTZA, PBG, PAS, SGP, WYE.

Table B.2: *Deepwater Horizon* Explosion Estimates, 3-Day Event Window

Variable	(1) CAR_{3d}		(2) CAR_{3d}		Mean $\widehat{CAR}_{3d}$	
Industry Fixed-Effect	Yes		Yes		-	
Environmental Performance	-0.01**	(<0.01)	-			
Banks and Insurance	-		-0.04	(0.03)	-1.10**	(0.43)
Basic Materials	-		-0.05*	(0.03)	1.84***	(0.48)
Consumer Products, Cars	-		<0.01	(0.01)	0.88*	(0.46)
Financial Services	-		-0.07**	(0.03)	0.32	(0.59)
Food & Beverage	-		0.05	(0.04)	-1.48***	(0.25)
General Industrials	-		0.01	(0.01)	0.88*	(0.49)
Health Care	-		-0.03	(0.03)	-0.65	(0.45)
Industrial Goods	-		-0.02*	(0.01)	0.79**	(0.37)
Media, Travel, Leisure	-		0.02	(0.03)	1.69**	(0.68)
Oil and Gas	-		-0.04	(0.05)	2.36***	(0.36)
Pharmaceuticals	-		0.03	(0.03)	0.00	(0.48)
Retail	-		0.08**	(0.04)	-0.47	(0.32)
Technology	-		-0.03*	(0.02)	0.14	(0.27)
Transportation, Aerospace	-		-0.06**	(0.02)	-0.41	(0.27)
Utilities	-		-0.02	(0.02)	0.31	(0.28)
Firm-Level						
ln (Mkt. Value)	-0.27*	(0.16)	-0.36**	(0.17)	-	
ln (Sales)	<0.01	(0.14)	0.09	(0.15)	-	
EpS	<0.01	(0.05)	<0.01	(0.06)	-	
N	484		484			
R^2	0.19		0.22			
F	6.81***		5.07***			

Notes: Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.
The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: ACS, BRK3, BDK, BKC, DTV, ETN, FAF, FO, GENZ, JAVA, KFT, LSTZA, PBG, PAS, SGP, WYE.

Table B.3: Responsibility Disclosure Estimates, 6-Day Event Window

Variable	(1) CAR_{6d}		(2) CAR_{6d}		Mean $\widehat{CAR}_{6d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	-0.03**	(0.01)	-			
Banks and Insurance	-		0.14**	(0.06)	5.98***	(0.67)
Basic Materials	-		0.02	(0.04)	3.03***	(1.08)
Consumer Products, Cars	-		-0.04*	(0.02)	-1.46**	(0.63)
Financial Services	-		-0.10*	(0.06)	2.59***	(0.82)
Food & Beverage	-		-0.03	(0.13)	-0.27	(0.60)
General Industrials	-		-0.03	(0.03)	-1.13	(0.93)
Health Care	-		-0.02	(0.04)	-2.66***	(0.55)
Industrial Goods	-		-0.05	(0.03)	-0.59	(0.63)
Media, Travel, Leisure	-		-0.04	(0.03)	-0.71	(0.61)
Oil and Gas	-		-0.04	(0.09)	1.69**	(0.69)
Pharmaceuticals	-		0.03	(0.04)	0.11	(0.75)
Retail	-		-0.03	(0.04)	-1.42***	(0.42)
Technology	-		0.02	(0.02)	-2.01***	(0.43)
Transportation, Aerospace	-		-0.02	(0.04)	-0.34	(0.48)
Utilities	-		-0.04	(0.03)	1.02**	(0.47)
Firm-Level						
ln (Mkt. Value)	0.20	(0.25)	0.23	(0.26)	-	
ln (Sales)	<0.02	(0.24)	0.01	(0.26)	-	
EpS	-0.26***	(0.06)	-0.29***	(0.07)	-	
N	483		483			
R^2	0.31		0.33			
F	10.17***		7.68***			

Notes: Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively. The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: ACS, BRK3, BDK, BJS, BKC, DTV, ETN, FAF, FO, GENZ, JAVA, KFT, LSTZA, PBG, PAS, SGP, WYE.

Table B.4: Oil-Spill Landfall Estimates, 9-Day Event Window

	(1)		(2)			
Variable	CAR_{9d}		CAR_{9d}		Mean $\widehat{CAR}_{9d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	0.02	(0.01)	-			
Banks and Insurance	-		0.02	(0.04)	-0.93*	(0.51)
Basic Materials	-		-0.04	(0.03)	-0.14	(0.63)
Consumer Products, Cars	-		0.05*	(0.03)	-2.16***	(0.81)
Financial Services	-		0.05	(0.04)	-0.97*	(0.56)
Food & Beverage	-		-0.16	(0.15)	1.92***	(0.56)
General Industrials	-		<0.01	(0.03)	-0.71	(0.77)
Health Care	-		0.11***	(0.03)	-0.71	(0.53)
Industrial Goods	-		-0.03	(0.02)	-1.26**	(0.50)
Media, Travel, Leisure	-		-0.02	(0.02)	-0.89	(0.68)
Oil and Gas	-		0.20	(0.15)	2.73**	(1.23)
Pharmaceuticals	-		-0.05	(0.08)	0.33	(0.83)
Retail	-		0.17***	(0.06)	-2.08***	(0.57)
Technology	-		0.04	(0.04)	-0.77	(0.56)
Transportation, Aerospace	-		0.04	(0.04)	-2.34***	(0.55)
Utilities	-		-0.02	(0.03)	1.07***	(0.37)
Firm-Level						
ln (Mkt. Value)	0.72***	(0.26)	0.67***	(0.26)	-	
ln (Sales)	-0.70***	(0.26)	-0.64**	(0.26)	-	
EpS	0.02	(0.06)	0.03	(0.07)	-	
N	483		483			
R^2	0.15		0.20			
F	4.54***		4.48***			

Notes: Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.
The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: ACS, BRK3, BDK, BJS, BKC, DTV, ETN, FAF, FO, GENZ, JAVA, KFT, LSTZA, PBG, PAS, SGP, WYE.

Table B.5: Hurricane Irene Estimates, 5-Day Event Window

Variable	(1) CAR_{5d}		(2) CAR_{5d}		Mean $\widehat{CAR}_{5d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	<0.01	0.01	-			
Banks and Insurance	-		0.02	(0.03)	0.59	(1.02)
Basic Materials	-		-0.01	(0.02)	1.04***	(0.31)
Consumer Products, Cars	-		-0.01	(0.02)	0.41	(0.42)
Financial Services	-		<0.01	(0.02)	-0.88*	(0.53)
Food & Beverage	-		0.08	(0.08)	0.65	(0.43)
General Industrials	-		-0.01	(0.02)	0.14	(0.53)
Health Care	-		0.03	(0.02)	0.24	(0.36)
Industrial Goods	-		-0.01	(0.02)	0.23	(0.43)
Media, Travel, Leisure	-		-0.01	(0.02)	1.10**	(0.46)
Oil and Gas	-		<0.01	(0.02)	1.64***	(0.38)
Pharmaceuticals	-		-0.01	(0.02)	0.99***	(0.32)
Retail	-		-0.04	(0.03)	-0.10	(0.46)
Technology	-		0.02	(0.02)	-0.02	(0.39)
Transportation, Aerospace	-		0.03***	(0.01)	-0.75**	(0.34)
Utilities	-		-0.04**	(0.01)	1.16***	(0.27)
Firm-Level						
ln (Mkt. Value)	-0.28	0.19	-0.31	(0.20)	-	
ln (Sales)	-0.04	0.22	-0.04	(0.23)	-	
EpS	-0.07*	0.04	-0.08*	(0.04)	-	
N	483		483			
R^2	0.08		0.10			
F	4.19***		3.33***			

Notes: Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively. The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: BEC, BDK, BKC, CAL, DTV, EKDKQ, ETN, FO, GENZ, HEW.Z, KFT, LSTZA, MI., Q, RGC, SII, and THC.

B.1.2 Half-Year Estimation Window Results

Table B.6: Iceland Volcano Estimates, 6-Day Event Window

	(1)		(2)			
Variable	CAR_{6d}		CAR_{6d}		Mean $\widehat{CAR}_{6d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	0.01	(0.01)	-			
Banks and Insurance	-		0.05*	(0.03)	1.17**	(0.48)
Basic Materials	-		0.11**	(0.05)	-2.63***	(0.79)
Consumer Products, Cars	-		-0.05**	(0.02)	2.05***	(0.50)
Financial Services	-		0.12**	(0.05)	-1.39**	(0.63)
Food & Beverage	-		-0.04	(0.07)	-0.52	(0.34)
General Industrials	-		-0.04	(0.03)	2.35***	(0.59)
Health Care	-		-0.07**	(0.03)	-0.75	(0.54)
Industrial Goods	-		0.04**	(0.02)	0.61	(0.43)
Media, Travel, Leisure	-		0.03	(0.05)	-0.40	(0.80)
Oil and Gas	-		0.06	(0.07)	0.69	(0.49)
Pharmaceuticals	-		0.05	(0.07)	-3.47***	(0.82)
Retail	-		0.02	(0.05)	0.30	(0.41)
Technology	-		-0.03	(0.02)	0.55	(0.46)
Transportation, Aerospace	-		0.06	(0.03)	1.79***	(0.60)
Utilities	-		-0.01	(0.02)	0.11	(0.36)
Firm-Level						
ln (Mkt. Value)	-0.11	(0.21)	-0.12	(0.21)	-	
ln (Sales)	0.02	(0.21)	0.10	(0.22)	-	
EpS	-0.10	(0.07)	-0.07	(0.06)	-	
N	484		484			
R^2	0.17		0.23			
F	4.70***		4.05***			

Notes: Estimation period is 13 October 2009 - 13 April 2010. Coefficient estimates are in percentage. The second column shows in each specification shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.
The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: ACS, BRK3, BDK, BKC, DTV, ETN, FAF, FO, GENZ, JAVA, KFT, LSTZA, PBG, PAS, SGP, WYE.

Table B.7: *Deepwater Horizon* Explosion Estimates, 3-Day Event Window

Variable	(1) CAR_{3d}		(2) CAR_{3d}		Mean $\widehat{CAR}_{3d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	-0.01	(0.01)	-			
Banks and Insurance	-		-0.06***	(0.02)	-2.22***	(0.40)
Basic Materials	-		-0.05*	(0.03)	1.68***	(0.49)
Consumer Products, Cars	-		0.01	(0.01)	0.62	(0.47)
Financial Services	-		-0.06**	(0.03)	-0.50	(0.59)
Food & Beverage	-		0.05	(0.05)	-1.32***	(0.26)
General Industrials	-		$<$0.01	(0.01)	0.45	(0.51)
Health Care	-		-0.03	(0.03)	-0.37	(0.46)
Industrial Goods	-		-0.02	(0.01)	0.46	(0.37)
Media, Travel, Leisure	-		0.02	(0.03)	1.11	(0.68)
Oil and Gas	-		-0.03	(0.05)	2.12***	(0.34)
Pharmaceuticals	-		0.02	(0.03)	0.22	(0.49)
Retail	-		0.08**	(0.04)	-0.64**	(0.31)
Technology	-		-0.03*	(0.02)	0.27	(0.29)
Transportation, Aerospace	-		-0.06***	(0.02)	-0.73***	(0.25)
Utilities	-		-0.02	(0.02)	0.36	(0.28)
Firm-Level						
ln (Mkt. Value)	-0.25	(0.16)	-0.36**	(0.17)	-	
ln (Sales)	$<$0.01	(0.14)	0.10	(0.15)	-	
EpS	0.03	(0.05)	0.03	0.05)	-	
N	484		484			
R^2	0.18		0.21			
F	7.23***		5.27***			

Notes: Estimation period is 20 October 2009 - 20 April 2010. Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.
The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: ACS, BRK3, BDK, BKC, DTV, ETN, FAF, FO, GENZ, JAVA, KFT, LSTZA, PBG, PAS, SGP, WYE.

Table B.8: Responsibility Disclosure Estimates, 6-Day Event Window

	(1)		(2)			
Variable	CAR_{6d}		CAR_{6d}		Mean $\widehat{CAR}_{6d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	-0.02*	(0.01)	-			
Banks and Insurance	-		0.09***	(0.03)	2.85***	(0.52)
Basic Materials	-		0.02	(0.04)	3.60***	(1.10)
Consumer Products, Cars	-		-0.02	(0.02)	-1.54**	(0.66)
Financial Services	-		-0.06	(0.05)	0.56	(0.69)
Food & Beverage	-		-0.01	(0.13)	0.79	(0.62)
General Industrials	-		-0.04	(0.03)	-1.71**	(0.85)
Health Care	-		-0.01	(0.05)	-0.81	(0.69)
Industrial Goods	-		-0.03	(0.03)	-0.89	(0.66)
Media, Travel, Leisure	-		-0.04	(0.03)	-1.72**	(0.66)
Oil and Gas	-		0.02	(0.11)	1.86**	(0.81)
Pharmaceuticals	-		0.01	(0.05)	1.47**	(0.72)
Retail	-		-0.02	(0.05)	-1.28***	(0.47)
Technology	-		$<$0.01	(0.03)	-0.57***	(0.45)
Transportation, Aerospace	-		-0.02	(0.03)	-0.58	(0.45)
Utilities	-		-0.04	(0.03)	1.78***	(0.42)
Firm-Level						
ln (Mkt. Value)	0.16	(0.27)	0.15	(0.29)	-	
ln (Sales)	0.03	(0.26)	0.07	(0.28)	-	
EpS	-0.17**	(0.07)	-0.18**	(0.07)	-	
N	483		483			
R^2	0.19		0.20			
F	6.11***		4.16***			

Notes: Estimation period is 20 October 2009 - 20 April 2010. Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.
The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: ACS, BRK3, BDK, BJS, BKC, DTV, ETN, FAF, FO, GENZ, JAVA, KFT, LSTZA, PBG, PAS, SGP, WYE.

Table B.9: Oil-Spill Landfall Estimates, 4-Day Event Window

	(1)		(2)			
Variable	CAR_{4d}		CAR_{4d}		Mean $\widehat{CAR}_{4d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	0.02**	(0.01)				
Banks and Insurance	-		-0.02	(0.02)	-0.90***	(0.30)
Basic Materials	-		-0.02	(0.02)	-1.75***	(0.47)
Consumer Products, Cars	-		0.02	(0.02)	-1.31***	(0.50)
Financial Services	-		0.03	(0.04)	-0.68	(0.45)
Food & Beverage	-		-0.05	(0.07)	1.05***	(0.33)
General Industrials	-		-0.02	(0.02)	-0.42	(0.45)
Health Care	-		0.12***	(0.02)	0.67*	(0.39)
Industrial Goods	-		-0.01	(0.01)	-1.07***	(0.30)
Media, Travel, Leisure	-		-0.01	(0.02)	0.01	(0.46)
Oil and Gas	-		0.23	(0.16)	1.06	(1.01)
Pharmaceuticals	-		0.04	(0.04)	-0.24	(0.58)
Retail	-		0.09***	(0.03)	-1.39***	(0.42)
Technology	-		0.02	(0.02)	0.82**	(0.32)
Transportation, Aerospace	-		0.01	(0.01)	-1.73***	(0.23)
Utilities	-		-0.02	(0.01)	-0.43**	(0.21)
Firm-Level						
ln (Mkt. Value)	0.32*	(0.19)	0.29	(0.18)	-	
ln (Sales)	-0.19	(0.20)	-0.16	(0.18)	-	
EpS	0.02	(0.06)	0.02	(0.05)	-	
N	483		483			
R^2	0.15		0.22			
F	7.15***		7.19***			

Notes: Estimation period is 20 October 2009 - 20 April 2010. Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.
The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: ACS, BRK3, BDK, BJS, BKC, DTV, ETN, FAF, FO, GENZ, JAVA, KFT, LSTZA, PBG, PAS, SGP, WYE.

Table B.10: Hurricane Irene Estimates, 5-Day Event Window

	(1)		(2)			
Variable	CAR_{5d}		CAR_{5d}		Mean $\widehat{CAR}_{5d}$	
Industry Fixed-Effects	Yes		Yes		-	
Environmental Performance	<0.01	(0.01)	-			
Banks and Insurance	-		0.02	(0.03)	0.40	(1.04)
Basic Materials	-		-0.01	(0.02)	0.76**	(0.31)
Consumer Products, Cars	-		-0.01	(0.02)	0.01	(0.44)
Financial Services	-		-0.01	(0.02)	-1.25**	(0.54)
Food & Beverage	-		0.07	(0.08)	0.19	(0.45)
General Industrials	-		-0.01	(0.02)	0.13	(0.56)
Health Care	-		0.02	(0.02)	-0.02	(0.37)
Industrial Goods	-		-0.01	(0.02)	0.01	(0.43)
Media, Travel, Leisure	-		-0.01	(0.02)	0.86*	(0.47)
Oil and Gas	-		<0.01	(0.02)	1.60***	(0.39)
Pharmaceuticals	-		-0.01	(0.02)	0.40	(0.33)
Retail	-		-0.04	(0.03)	-0.45	(0.45)
Technology	-		0.02	(0.02)	-0.13	(0.38)
Transportation, Aerospace	-		0.03***	(0.01)	-1.09***	(0.36)
Utilities	-		-0.04**	(0.01)	0.51*	(0.26)
Firm-Level						
ln (Mkt. Value)	-0.32*	(0.19)	-0.35*	(0.20)	-	
ln (Sales)	0.02	(0.22)	0.01	(0.23)	-	
EpS	-0.08*	(0.04)	-0.08*	(0.04)	-	
N	483		483			
R^2	0.07		0.09			
F	2.92***		2.45***			

Notes: Estimation period is 25 February 2011 - 26 August 2011. Coefficient estimates are in percentage. The second column in specification 2 shows estimated CARs by industry, evaluated at the industry mean. Robust standard errors in parantheses. Significance of statistic at the 1%, 5%, and 10% level is denoted by ***, **, and *, respectively.
The following companies (tickers) were excluded from the regression either because of incomplete accounting data or because of incomplete financial time series: BEC, BDK, BKC, CAL, DTV, EKDKQ, ETN, FO, GENZ, HEW.Z, KFT, LSTZA, MI., Q, RGC, SII, and THC.

Appendix C

Uncertainty and the WTA-WTP Disparity

C.1 Instructions

C.1.1 WTP Treatment

The instructions below correspond with the (1) Training Tasks, (2) Private Good High Stakes Tasks, (3) Public Good High Stakes Tasks treatment order.

C.1.1.1 Low Stakes Rounds

You are about to participate in an experiment in economic decision making. Please follow the instructions carefully, as the amount of money you earn in the experiment will depend on your decisions. At the end of today's session, you will be paid your earnings in private and in cash. Please do not communicate with other participants during the experiment unless instructed. Importantly, please refrain from verbally reacting to events that occur during the experiment.

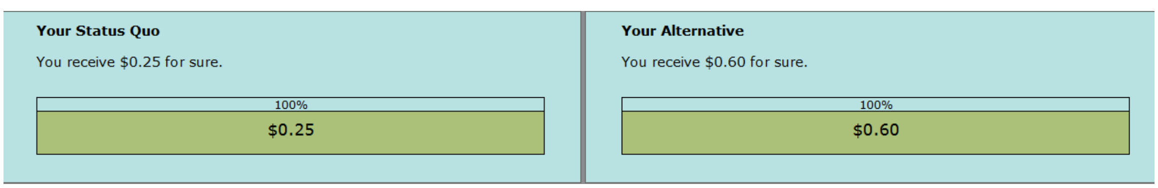
Overview

Today's experiment will involve several buying tasks. Each task may impact your earnings, which means that it is very important to consider each task carefully prior to making a decision. Each task is independent from the other tasks, so the decision you make in one task will not affect the outcome or earnings of any other task. All money amounts are in U.S. Dollars.

There are three parts to the experiment. The first part involves low stakes buying tasks. The second and third parts involve high stakes buying tasks. You will receive additional instructions at the end of the first part.

Part 1: Individual Buying Task (Low Stakes)

The buying task works as follows. To start out, you own the Status Quo, which is a "good" worth an amount of money to you. (In the example below, the Status Quo is $0.25.) The experimenter wishes to sell an Alternative to you instead of the Status Quo. The Alternative is a different "good" worth an amount of money. (In the example below, the alternative is $0.60.)



Your task is to submit a bid to buy the Alternative from the experimenter.

If your bid is accepted, you will own the Alternative instead of the Status Quo. You will pay the experimenter for buying the Alternative. Your earnings from this task will be equal to the Alternative minus the buying price: **Earnings = Alternative – Buying Price**.

If your bid is not accepted, you will keep the Status Quo. Your earnings from this task will be equal to the Status Quo: **Earnings = Status Quo**.

As you will see, your best strategy is to determine the maximum you are willing to pay to own the Alternative instead of the Status Quo and bid this amount. This maximum amount is the price at which you feel just as well off owning the Alternative (and paying this price) as with owning the Status Quo.

After you submit your bid, it will be compared to a **randomly determined price**. Since the price is random, it is neither based on your bid nor is it related to the bids of any other person in the room.

Here is how this random price will be used to determine whether you buy:

If your bid is *greater than or the same* as the random price, then you buy the Alternative and give up the Status Quo. **But here is the interesting part: the buying price is the random price, not your bid**. What this means is that as long as you bid the maximum amount that owning the Alternative rather than the Status Quo is worth to you, you will only buy at prices that are favorable to you and not buy at prices unfavorable to you.

If your bid is *less than* the random price then you do not buy the Alternative. Thus, you simply keep the Status Quo. Since you did not buy, you do not pay the random price.

As you can see from the example, the Status Quo is worth money to you and so you may be asking yourself: **"Why would I want pay a price to buy the Alternative and give up the Status Quo?"** By buying, as long as it is at a favorable price to you, **you can earn more money** than by simply keeping the Status Quo. Using the example from before, let us see how:

- Suppose the random price is \$0.10 and your bid is greater than \$0.10. You would own the Alternative (worth \$0.60) and pay the random price of \$0.10.

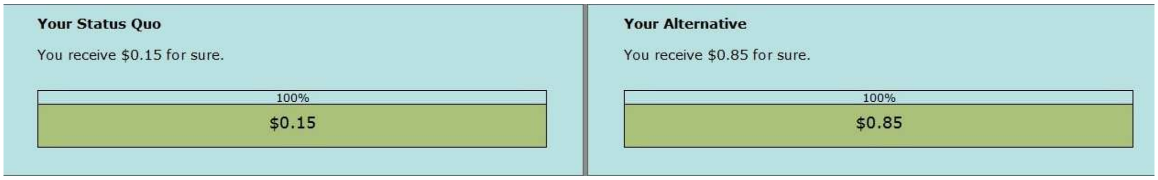
Your earnings would be \$0.50, which is more than simply keeping the Status Quo (\$0.25).

- Of course, you will not want to buy at just any price. Suppose the random price is \$0.50 and your bid is greater than \$0.50. You would own the Alternative (worth \$0.60) and pay the price of \$0.50 ... for a total of \$0.10. This amount is less than the Status Quo (\$0.25)!

How do you determine your bid? One way is to start with the minimum possible price (say \$0.01) and then ask yourself: "Do I want to buy at a price of \$0.01?" (If you do, your earnings would be \$0.60 – \$0.01 = \$0.59 instead of \$0.25 (Status Quo)). Then, work your way up: "Do I want to buy at a price of \$0.02?" (If you do, your earnings would be \$0.60 – \$0.02 = \$0.58 instead of \$0.25). You will eventually reach a price at which you are just as well off as with the Status Quo. Alternatively, you can start with the maximum possible price (say \$1.10) and then ask yourself: "Do I want to buy at a price of \$1.10?" (If you do, your earnings would be \$0.60 – \$1.10 = (-\$0.50) instead of \$0.25 (Status Quo)). Then, work your way down: "Do I want to buy at a price of \$1.09?" and so on. **It is your best strategy to bid a price at which you feel just as well off owning the Alternative (and paying this price) as with owning the Status Quo.**

Before we proceed, are there any questions?

Below is an example buying task. To make sure you understand how earnings are determined, we ask you, hypothetically, to make a bid as if this were a paid decision task. Based on this bid we then ask you to answer questions related to what you would have earned from this task. We will pay you **\$2** if you answer **all** earnings questions correctly, and add this to your earnings in today's experiment.



Based on the Status Quo and Alternative above, please place a bid: ______

Q1. Suppose the random price is $1. You would (circle one): not buy buy
Your earnings from this task would then be: ______

Q2. Suppose the random price is $0.25. You would (circle one): not buy buy
Your earnings from this task would then be: ______

Q3. At what price would you be just as well off as with the Status Quo? ______

Please raise your hand when you are ready, so that we can check your calculations.

Ready to Go

You will now go through 1 practice buying task and 6 paid buying tasks. The practice task will be first and will not affect your earnings. You will be paid based on your decision in each of the 6 paid buying tasks that follow.

In each task, you can submit a bid between $0.00 and $1.10 (inclusive) using one-cent increments. Use a decimal point (".") to separate dollars and cents.

After you submit a bid, the computer will randomly determine the price by selecting a number between $0.01 and $1.10 (inclusive). Each number in this range has an equal chance of being selected.

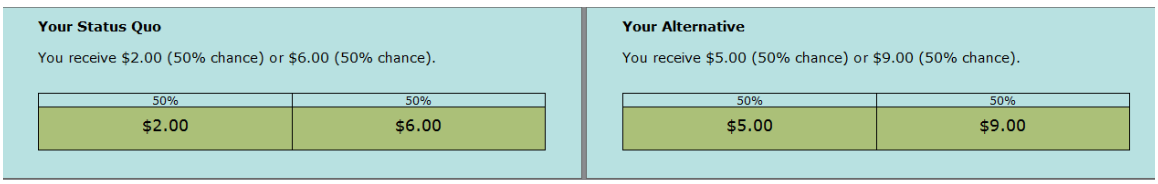
Before we proceed, are there any questions?

C.1.1.2 Private Good High Stakes Rounds

Part 2: Individual Buying Task (High Stakes)

What the Status Quo and Alternative are worth is now uncertain.

The Status Quo or the Alternative, or both, may now be worth an uncertain amount of money. In this event, you will see two possible amounts, each with a 50% chance of occurring. In the example below, you have a Status Quo where you receive \$2 or \$6, each with a 50% chance of occurring.



With an uncertain payout, to determine your earnings the computer will virtually "flip a coin" by generating a random number to determine which of the two amounts you will receive. For example, if you kept the Status Quo in the above example, if the virtual "flip" comes up Heads you would receive \$2 and if it comes up Tails you would receive \$6. Hence, each amount is equally likely.

The procedure that determines whether you buy is the same as before.

After you submit your bid, your bid will be compared to a **randomly determined price**. Since the price is random, it is neither based on your bid nor is it related to the bids of any other person in the room. Here is how this random price will be used to determine whether you buy:

If your bid is *greater than or the same* as the random price, then you buy the Alternative and give up the Status Quo. Your earnings from this task will equal the amount that your Alternative is worth, minus the random price.

If your bid is *less than* the random price then you do not buy the Alternative. Thus, you simply keep the Status Quo. Your earnings from this task will equal the amount that your Status Quo is worth.

In each task, you can submit a bid between \$0.00 and \$12.00 (inclusive) using one-cent increments. Use a decimal point (".") to separate dollars and cents.

After you submit a bid, the computer will randomly determine the price by selecting a number between \$0.01 and \$12.00 (inclusive). Each number in this range will have an equal chance of being selected.

At the end of today's session, to determine your earnings from this Part of the experiment, the computer will randomly select **at least one** of the tasks from Part 2 to be paid out.

You will not know the number of tasks in Part 2 until it is finished. The first task will be for practice and will not impact your earnings.

The remaining tasks can all affect your earnings. Since you will not know what tasks are played out to determine your earnings, you will not see any results after submitting an offer for a task. You will simply proceed to the next task. **Treat each task as if it determines your earnings.**

Before we proceed are there any questions?

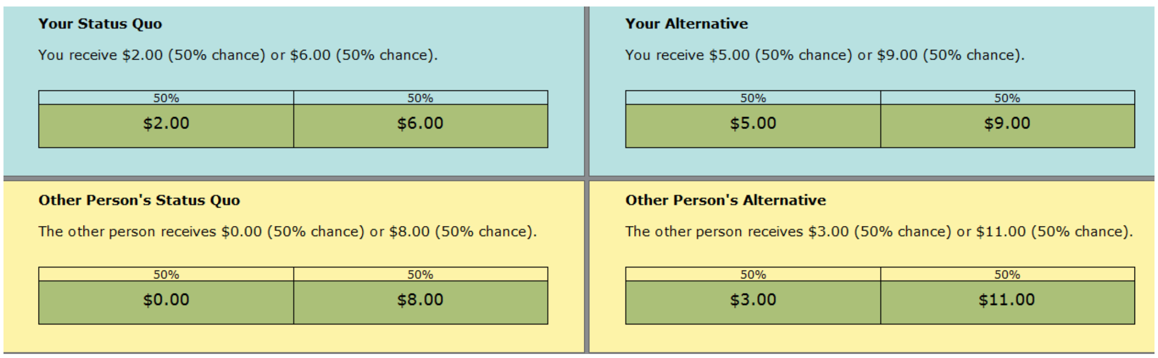
C.1.1.3 Public Good High Stakes Rounds

Part 3: Buying Task (High Stakes)

Your decisions will now affect the earnings of others.

In each task you will now be randomly and anonymously **matched with one other person in the room**. You will be re-matched with a different person prior to each task.

You will see the Status Quo and Alternative for yourself and the other person, as illustrated below. The amounts for the other person may be different from your own, so please look at this information carefully. Importantly, for these tasks **your bid determines whether or not you and the other person buy** the Alternative and give up the Status Quo. The person you are matched with **will not** have an opportunity to affect the outcome.



Similar to before, after you submit your bid, it will be compared to a **randomly determined price**. Since the price is random, it is neither based on your bid nor is it related to the bids of any other person in the room.

If your bid is *greater than or the same* as the random price, then both you and the other person buy the Alternative and give up Status Quo. Your earnings from this task will equal the amount that your Alternative is worth, minus the random price. The earnings for the other person will equal the amount that their Alternative is worth, minus the random price. You and the other person will each pay the random price (not your bid!).

If your bid is *less than* the random price, then both you and the other person keep the Status Quo. Your earnings from this task will equal the amount that your Status Quo is worth. The earnings for the other person will equal the amount that their Status Quo is worth.

In each task, you can submit a bid between \$0.00 and \$12.00 (inclusive) using one-cent increments. Use a decimal point (".") to separate dollars and cents.

After you submit a bid, the computer will randomly determine the price by selecting a number between \$0.01 and \$12.00 (inclusive). Each number in this range will have an equal chance of being selected.

Know that at the same time you are making a decision that affects the earnings of you and another person in the room, the person you are matched with is also making a decision that can affect your earnings.

At the end of today's session, to determine your earnings from this Part of the experiment, the computer will randomly select **at least two** of the tasks from Part 3 to be paid out. For at least one of these paid tasks, **your decision** will determine the earnings for you and the person you are matched with. For at least one of the paid tasks, the **decision of the person you are matched with** will determine the earnings for you and that person.

You will not know the number of tasks in Part 3 until it is finished. The first task will be for practice and will not impact your earnings.

The remaining tasks can all affect your earnings. Since you will not know what tasks are played out to determine your earnings, you will not see any results after submitting an offer for a task. You will simply proceed to the next task. **Treat each task as if it determines your earnings.**

Before we proceed are there any questions?

C.1.2 WTA Treatment

The instructions below correspond with the (1) Training Tasks, (2) Public Good High Stakes Tasks, (3) Private Good High Stakes Tasks treatment order.

C.1.2.1 Low Stakes Rounds

You are about to participate in an experiment in economic decision making. Please follow the instructions carefully, as the amount of money you earn in the experiment will depend on your decisions. At the end of today's session, you will be paid your earnings in private and in cash. Please do not communicate with other participants during the experiment unless instructed. Importantly, please refrain from verbally reacting to events that occur during the experiment.

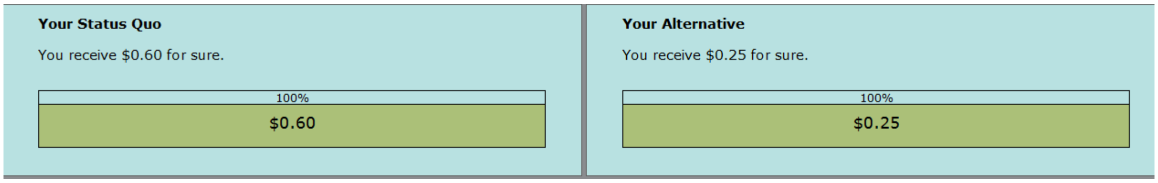
Overview

Today's experiment will involve several selling tasks. Each task may impact your earnings, which means that it is very important to consider each task carefully prior to making a decision. Each task is independent from the other tasks, so the decision you make in one task will not affect the outcome or earnings of any other task. All money amounts are in U.S. Dollars.

There are three parts to the experiment. The first part involves low stakes selling tasks. The second and third parts involve high stakes selling tasks. You will receive additional instructions at the end of the first part.

Part 1: Individual Buying Task (Low Stakes)

The selling task works as follows. You begin by owning the Status Quo, which is a "good" worth an amount of money to you. (In the example below, the Status Quo is \$0.60.) The experimenter wishes to buy your Status Quo and instead give you an Alternative. The Alternative is a different "good" worth an amount of money. (In the example below, the Alternative is $0.25.)



Your task is to submit an offer to sell the Status Quo to the experimenter.

If your offer is accepted, you will own the Alternative instead of the Status Quo. You will receive money from the experimenter for selling the Alternative. Your earnings from this task will be equal to the Alternative plus the selling price: **Earnings = Alternative + Selling Price**.

If your offer is not accepted, you will keep the Status Quo. Your earnings from this task will be equal to the Status Quo: **Earnings = Status Quo**.

As you will see, your best strategy is to determine the minimum compensation you are willing to accept to own the Alternative instead of the Status Quo and offer this amount. This minimum amount is the price at which you feel just as well off owning the Alternative (and receiving this price) as with owning the Status Quo.

After you submit your offer, it will be compared to a **randomly determined price**. Since the price is random, it is neither based on your bid nor is it related to the bids of any other person in the room.

Here is how this random price will be used to determine whether you buy:

If your offer is *less than or the same* as the random price, then you sell the Status Quo and instead own the Alternative. **But here is the interesting part: the selling price is the random price, not your offer**. What this means is that as long as you offer the minimum compensation required to own the Alternative instead of the Status Quo, you will only sell at prices that are favorable to you and not sell at prices unfavorable to you.

If your offer is *more than* the random price then you do not sell the Status Quo. Thus, you simply keep the Status Quo. Since you did not sell, you do not receive the random price.

As you can see from the example, the Alternative is worth less money to you than the Status Quo and so you may be asking yourself: **"Why would I want to own the Alternative and sell the Status Quo?"** By selling, as long as it is at a favorable price to you, **you can earn more money** than by simply keeping the Status Quo. Using the example from before, let us see how:

- Suppose the random price is \$0.75, and your offer is less than \$0.75. You would own the Alternative (worth \$0.25) and get paid the random price of \$0.75. Your earnings would be \$1, which is more than simply keeping the Status Quo (\$0.60).

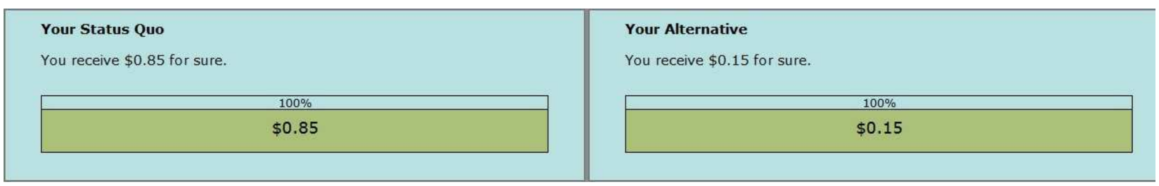
- Of course, you will not want to sell at just any price. Suppose the random price is \$0.10 and your offer is less than \$0.10. You would own the Alternative (worth \$0.25) and receive the random price of \$0.10... for a total of \$0.35. This amount is less than the Status Quo (\$0.60)!

How do you determine your offer? One way is to start with the minimum possible price (say \$0.00) and then ask yourself: "Do I want to sell at a price of \$0.00?" (If you do, your earnings would be \$0.25 + \$0.00 = \$0.25 instead of \$0.60 (Status Quo)). Then, work your way up: "Do I want to sell at a price of \$0.01?" (If you do, your earnings would be \$0.25 + \$0.01 = \$0.26 instead of \$0.60). You will eventually reach a price at which you are just as well off as with the Status Quo. Alternatively, you can start with the maximum possible price (say it is \$1.09) and then ask yourself "Do I want to sell at a price of \$1.09?" (If you do your earnings would be \$0.25 + \$1.09

= \$1.34 instead of \$0.60 (Status Quo)). Then work your way down: “Do I want to sell at a price of \$1.08?” and so on. **It is your best strategy to offer a price at which you feel just as well off owning the Alternative (and receiving this price) as with owning the Status Quo.**

Before we proceed, are there any questions?

Below is an example selling task. To make sure you understand how earnings are determined, we next ask you, hypothetically, to make an offer as if this were a paid decision task. Based on this offer we then ask you to answer questions related to what you would have earned from this task. We will pay you **\$2** if you answer **all** earnings questions correctly, and add this to your earnings in today's experiment.



Based on the Status Quo and Alternative above, please make an offer: ______

Q1. Suppose the random price is \$1. You would (circle one): not sell sell
Your earnings from this task would then be: ______

Q2. Suppose the random price is \$0.25. You would (circle one): not sell sell
Your earnings from this task would then be: ______

Q3. At what price would you be just as well off as with the Status Quo? ______

Please raise your hand when you are ready, so that we can check your calculations.

Ready to Go

You will now go through 1 practice selling task and 6 paid selling tasks. The practice task will be first and will not affect your earnings in any way. You will be paid based on your decision in each of the 6 paid selling tasks that follow.

In each task, you can submit an offer between \$0.00 and \$1.10 (inclusive) using one-cent increments. Use a decimal point (".") to separate dollars and cents.

After you submit an offer, the computer will randomly determine the price by selecting a number between \$0.00 and \$1.09 (inclusive). Each number in this range has an equal chance of being selected.

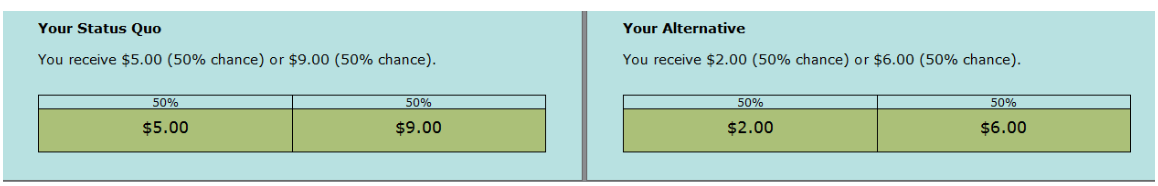
Before we proceed, are there any questions?

C.1.2.2 Group High Stakes Rounds

Part 2: Selling Task (High Stakes)

Your decisions will now affect the earnings of others.

The Status Quo or the Alternative, or both, may now be worth an **uncertain** amount of money. In this event, you will be shown two possible amounts, each with a 50% chance of occurring. In the example below, you have a Status Quo where you receive \$5 or \$9, each with a 50% chance of occurring.

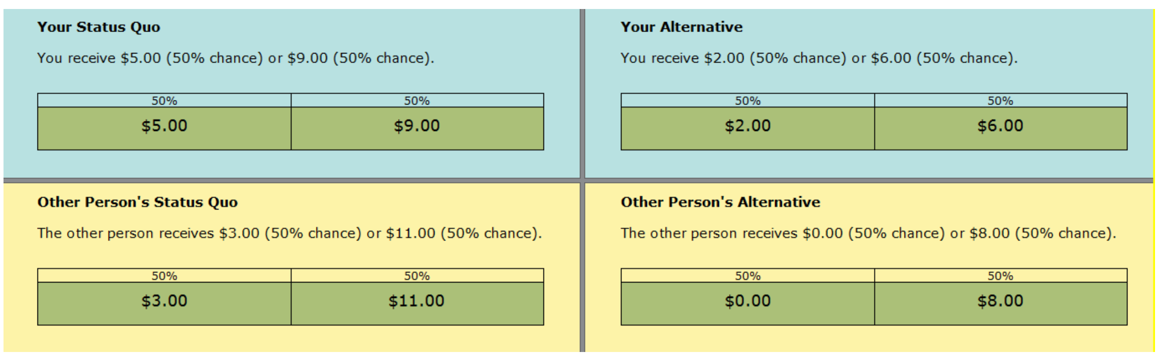


With an uncertain payout, to determine your earnings the computer will virtually "flip a coin" by generating a random number to determine which of the two amounts you will receive. For example, if you kept the Status Quo in the above example, if the virtual "flip" comes up Heads you would receive \$5 and if it comes up Tails you would receive \$9. Hence each amount is equally likely.

Your decisions will now affect the earnings of others.

In each task you will now be randomly and anonymously **matched with one other person in the room**. You will be re-matched with a different person prior to each task.

You will see the Status Quo and Alternative for yourself and the other person, as illustrated below. The amounts for the other person may be different from your own, so please look at this information carefully. Importantly, for these tasks **your offer determines whether or not you and the other person will sell** the Status Quo and instead receive the Alternative. The person you are matched with **will not** have an opportunity to affect the outcome.



Similar to before, after you submit your offer, it will be compared to a **randomly determined price**. Since the price is random, it is neither based on your offer nor is it related to the offers of any other person in the room.

If your offer is *less than or the same* as the random price, then both you and the other person sell the Status Quo and instead own the Alternative. Your earnings from this task will equal the amount that your Alternative is worth, plus the random price. The earnings for the other person will equal the amount that their Alternative is worth, plus the random price. You and the other person will each receive the random price (not your offer!).

If your offer is *greater than* the random price, then both you and the other person keep the Status Quo. Your earnings from this task will equal the amount that your Status Quo is worth. The earnings for the person will equal the amount that their Status Quo is worth.

In each task, you can submit an offer between \$0.00 and \$12.00 (inclusive) using one-cent increments. Use a decimal point (".") to separate dollars and cents.

After you submit an offer, the computer will randomly determine the price by selecting a number between \$0.00 and \$11.99 (inclusive). Each number in this range will have an equal chance of being selected.

Know that at the same time you are making a decision that affects the earnings of you and another person in the room, the person you are matched with is also making a decision that can affect your earnings.

At the end of today's session, to determine your earnings from this Part of the experiment, the computer will randomly select **at least two** of the tasks from Part 2 to be paid out. For at least one of these paid tasks, **your decision** will determine the earnings for you and the person you are matched with. For at least one of the paid tasks, the **decision of the person you are matched with** will determine the earnings for you and that person.

You will not know the number of tasks in Part 2 until it is finished. The first task will be for practice and will not impact your earnings.

The remaining tasks can all affect your earnings. Since you will not know what tasks are played out to determine your earnings, you will not see any results after submitting an offer for a task. You will simply proceed to the next task. **Treat each task as if it determines your earnings.**

Before we proceed are there any questions?

C.1.2.3 Individual High Stakes Rounds

Part 3: Individual Selling Task (High Stakes)

Your decisions will no longer affect the earnings of others.

Unlike in the last part, you **will not** be randomly matched with another person in the room.

You will only see your Status Quo and your Alternative. Your decisions will only affect your earnings and not the earnings of another person.

The main rules are the same as before. We will go through these again.

After you submit your offer, your offer will be compared to a **randomly determined price**. Since the price is random, it is neither based on your offer nor is it related to the offers of any other person in the room. Here is how this random price will be used to determine whether you sell:

If your offer is *less than or the same* as the random price, then you sell the Status Quo and instead own the Alternative. Your earnings from this task will then be equal to the amount that your Alternative is worth, plus the random price.

If your offer is *greater than* the random price then you do not sell the Status Quo. Thus, you simply keep the Status Quo. Your earnings from this task will equal the amount that your Status Quo is worth.

In each task, you can submit an offer between \$0.00 and \$12.00 (inclusive) using one-cent increments. Use a decimal point (".") to separate dollars and cents.

After you submit a bid, the computer will randomly determine the price by selecting a number between \$0.00 and \$11.99 (inclusive). Each number in this range will have an equal chance of being selected.

At the end of today's session, to determine your earnings from this Part of the experiment, the computer will randomly select **at least one** of the tasks from Part 3 to be paid out.

You will not know the number of tasks in Part 3 until it is finished. The first task will be for practice and will not impact your earnings.

The remaining tasks can all affect your earnings. Since you will not know what tasks are played out to determine your earnings, you will not see any results after submitting an offer for a task. You will simply proceed to the next task. **Treat each task as if it determines your earnings.**

Before we proceed are there any questions?

C.2 Screen Shots

C.2.1 WTP Treatment

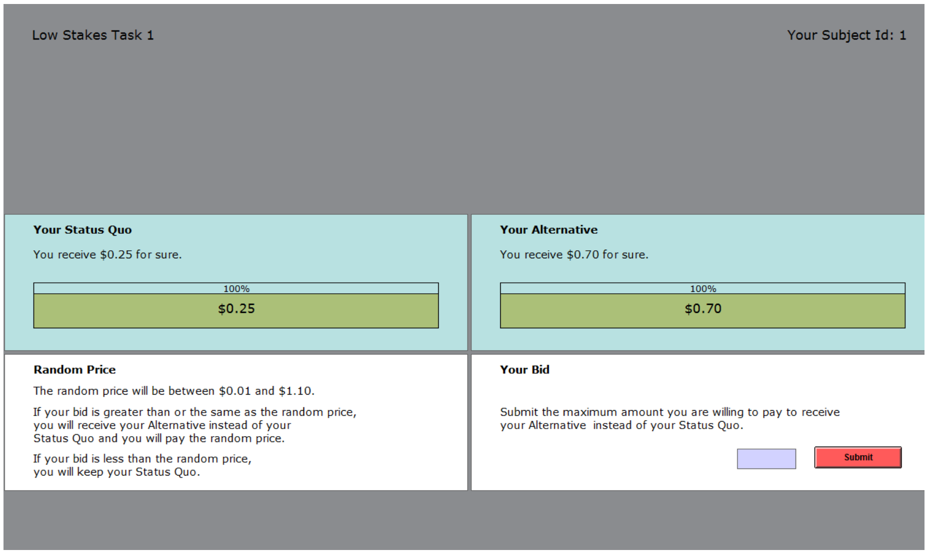


Figure C.1: Decision Screen, Training Buying Task

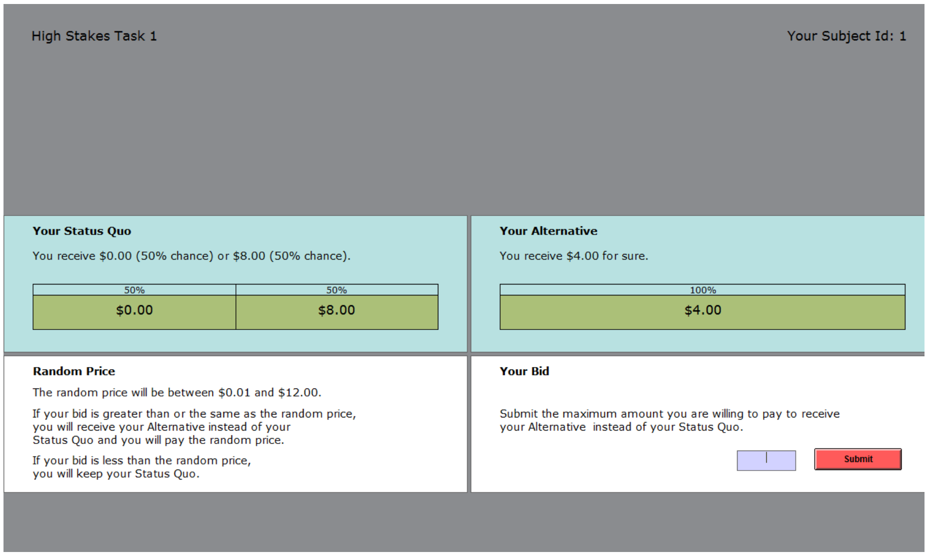


Figure C.2: Decision Screen, Private Good High Stakes Buying Task

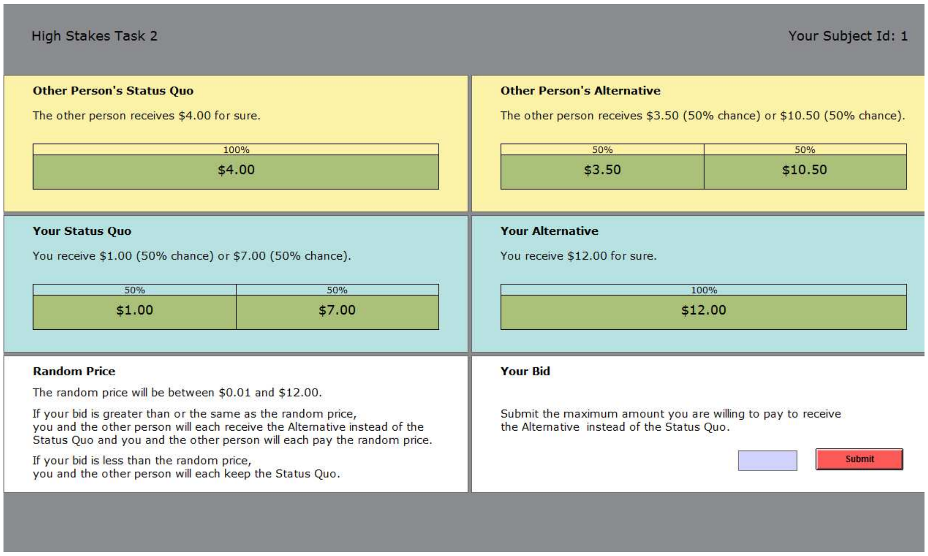


Figure C.3: Decision Screen, Public Good High Stakes Buying Task

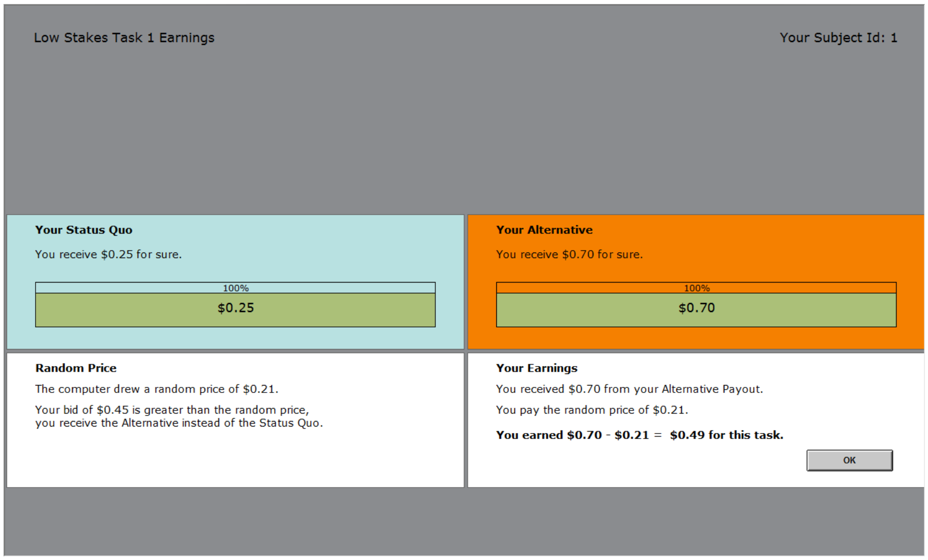


Figure C.4: Earnings Screen, Low Stakes Buying Task

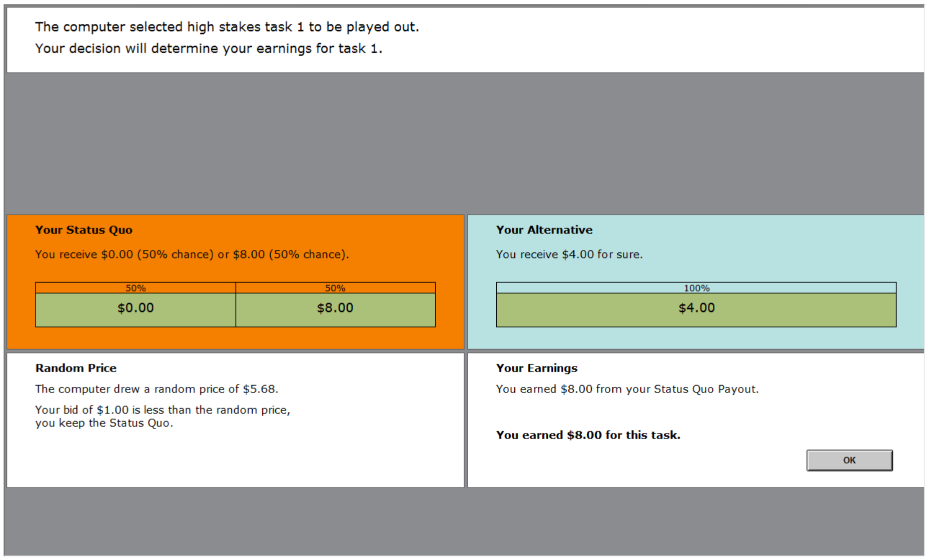


Figure C.5: Earnings Screen, Private Good High Stakes Buying Task

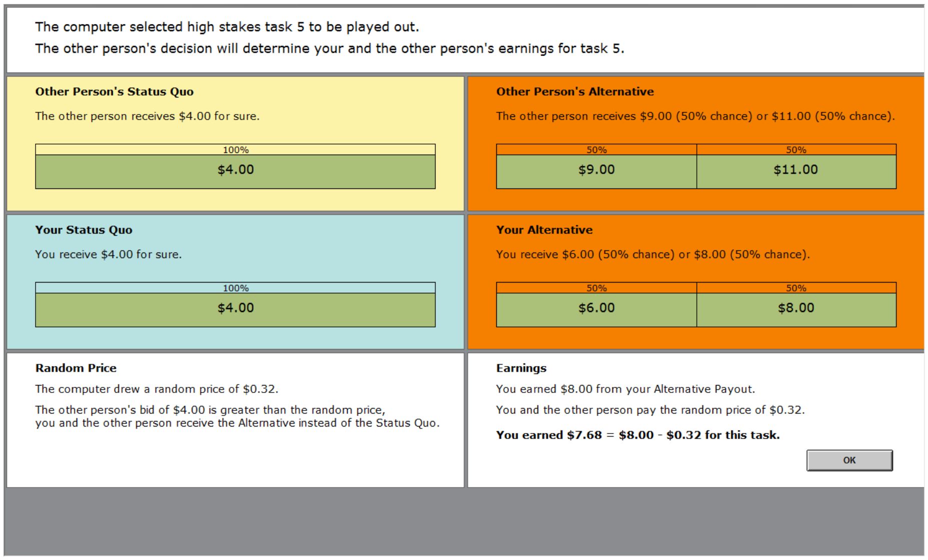


Figure C.6: Earnings Screen, Public Good High Stakes Buying Task

C.2.2 WTA Treatment

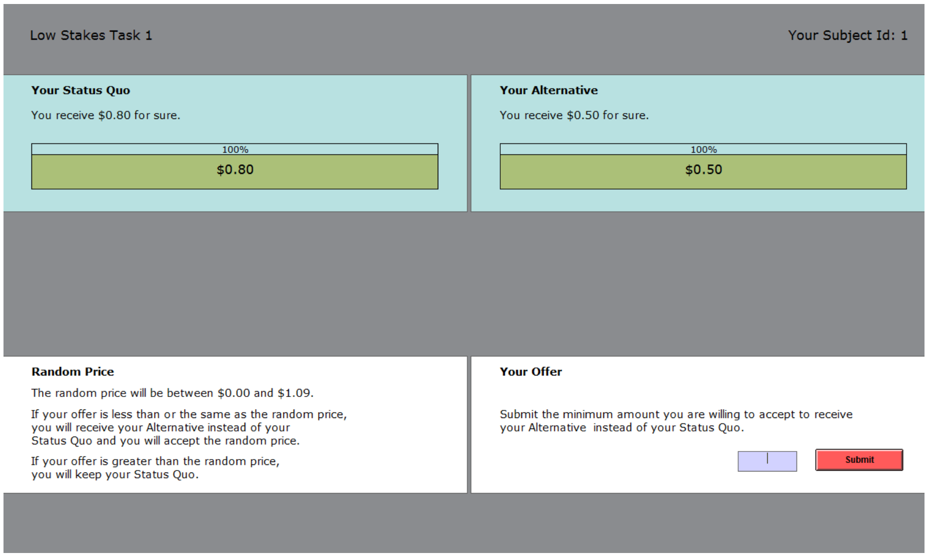


Figure C.7: Decision Screen, Low Stakes Selling Task

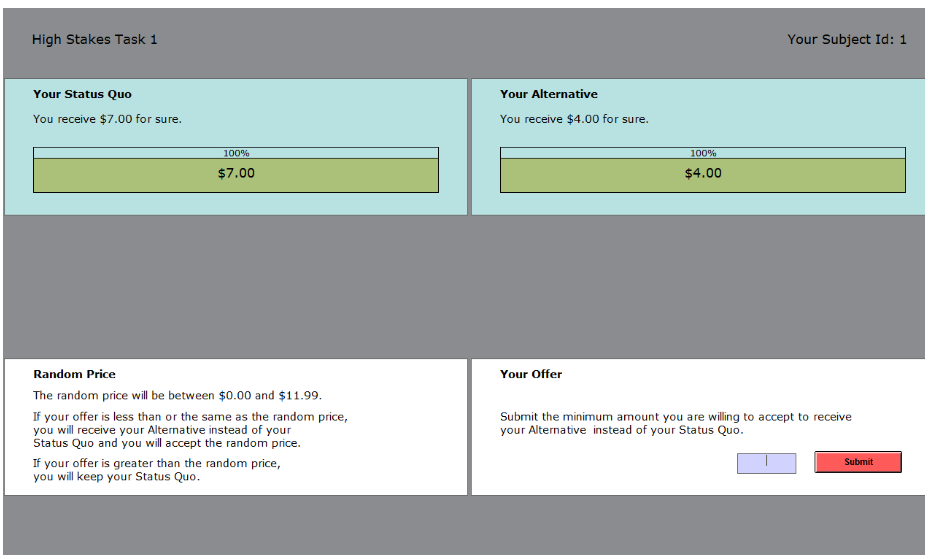


Figure C.8: Decision Screen, Private Good High Stakes Selling Task

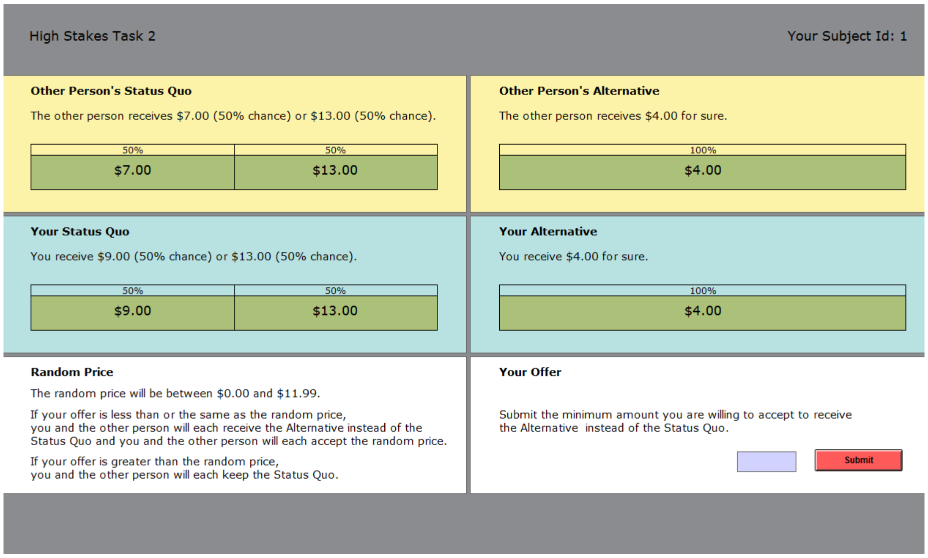


Figure C.9: Decision Screen, Public Good High Stakes Selling Task

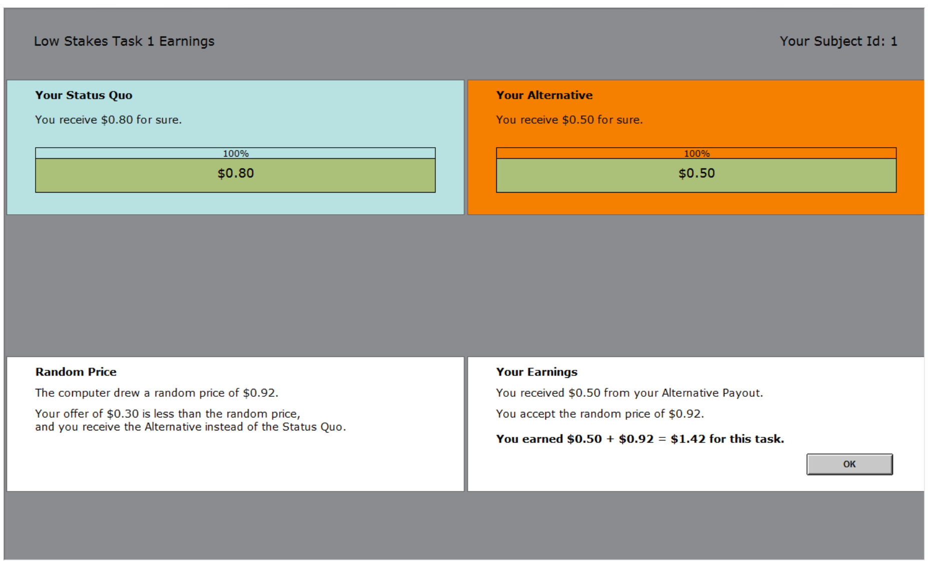


Figure C.10: Earnings Screen, Low Stakes Selling Task

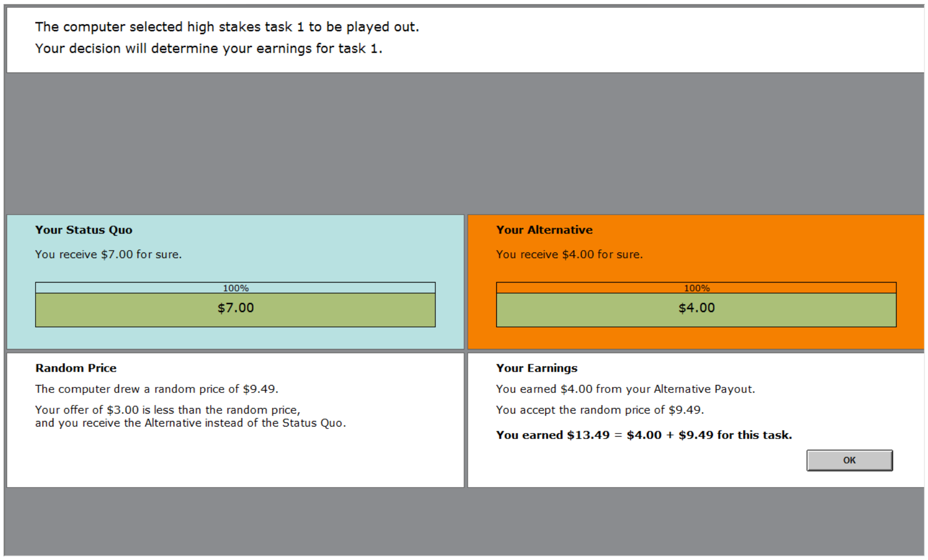


Figure C.11: Earnings Screen, Private Good High Stakes Selling Task

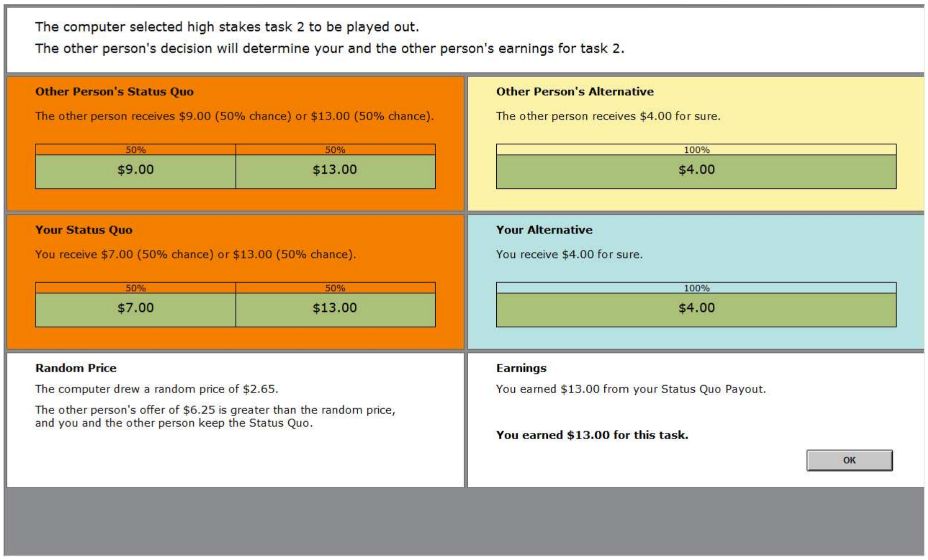


Figure C.12: Earnings Screen, Public Good High Stakes Selling Task

C.3 Regression Estimates

Table C.1: Public Good Regression Estimates, Full Sample

		Variable	$(o - \Delta\mu)$		$(b - \Delta\mu)$		Difference	
Homogeneous	Private	Intercept	1.49***	(0.24)	0.32*	(0.18)	1.17***	(0.29)
		$\Delta\sigma^{\oplus}$	-0.02	(0.13)	-0.21**	(0.08)	0.19	(0.15)
		$\Delta\sigma^{\ominus}$	0.68***	(0.10)	0.38***	(0.07)	0.30**	(0.12)
	Public	Intercept	1.69***	(0.28)	0.50**	(0.20)	1.19***	(0.35)
		$\Delta\sigma^{\oplus}$	-0.06	(0.11)	-0.33***	(0.09)	0.27*	(0.14)
		$\Delta\sigma^{\ominus}$	0.67***	(0.09)	0.29***	(0.08)	0.37***	(0.11)
Heterogeneous	Public	Intercept	2.52***	(0.31)	0.80***	(0.26)	1.73***	(0.40)
		$\Delta\sigma^{\oplus}$	-0.38***	(0.10)	-0.13*	(0.08)	-0.25*	(0.13)
		$\Delta\sigma^{\ominus}$	0.51***	(0.07)	0.23***	(0.07)	0.29***	(0.10)
		$\delta\mu^{\oplus}$	-0.39***	(0.05)	-0.38***	(0.04)	-0.01	(0.06)
		$\delta\mu^{\ominus}$	0.24***	(0.05)	0.15***	(0.04)	0.08	(0.06)
		$\delta\sigma^{\oplus}$	0.30***	(0.09)	0.06	(0.07)	0.24**	(0.11)
		$\delta\sigma^{\ominus}$	0.06	(0.06)	0.09	(0.06)	-0.02	(0.08)
$N = 5,208,\ R^2 = 0.38,\ F = 33.92$***								

Note: Coefficients estimates based on pooled model. WTA and WTP specific parameters are shown across columns for comparison. All coefficient estimates are in dollars. Robust standard errors, clustered at the subject level (168 subjects), are in parantheses. Significance of statistic at the 1%, 5%, and 10% is denoted by ***, **, and *, respectively.

Vita

Jens Schubert was born in Leipzig, Germany on February 10, 1980. He graduated from Leibniz High School in Leipzig in 1998. Jens studied Econophysics at the University of Ulm in Germany, where in 2006, he received a Master of Science in Econophysics (Dipl. Phys.-Oec.). His Master's thesis under the supervision of Dr. Hartmut Bertschat, Dr. William Brewer, and Dr. Mark Prandolini, studied the sign of magnetic hyperfine fields of cadmium atoms on nickel surfaces. His research led to a co-authored article in the *The European Physical Journal B*. He later studied Applied and Resource Economics at East Carolina University, obtaining a Master of Science in Economics in 2006. His thesis advisors, Dr. Jamie Brown-Kruse and Dr. Craig Landry, helped to spark Jens' interest in the field of Experimental Economics. Jens decided to continue graduate studies in Resource Economics at the University of Nevada, Reno, where he co-authored an article in the *American Journal of Agricultural Economics* with Tigran Melkonyan. Jens then studied Economics at the University of Tennessee, Knoxville, focusing on Environmental Economics, Industrial Organization, and Experimental Economics in pursuit of his Doctor of Philosophy degree. Jens has accepted a position as an Assistant Professor at Virginia Commonwealth University in Richmond, Virginia, where he will begin work in August 2013.