

**Under-contribution to generic advertising due to self-interested inequity aversion.**

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## **Under-contribution to generic advertising due to self-interested inequity aversion**

### **Abstract**

We modify Fehr and Schmidt's (1999) behavioural postulate of self-centred inequity aversion to explain producers' reluctance to fund generic fruit and vegetable advertising as a result of experiencing negative utility when others benefit more from a public good than themselves, but positive utility when they earn more than others. We find that higher variability in returns decreases the probability of a favourable vote. Conversely, if information about payoffs is incomplete, if subjects are allowed to experience a trial run of a generic advertising program, if returns are equal across producers, or if there is government support for the program, the likelihood of approval rises.

*Keywords:* Inequity Aversion, Generic Advertising, Experimental Economics.

*JEL Classification:* H41, M37, Q13.

## 1. Under-contribution to generic advertising due to self-interested inequity aversion

In the United States, a typical specific commodity checkoff program collects funds from producers of a particular agricultural commodity and uses those funds to improve the market position of the covered commodity by expanding markets, increasing demand, and developing new uses and markets. The idea of a mandatory checkoff program for broad-based advertising, which is designed to promote overall consumption of fruits and vegetables rather than specific commodities, is highly controversial among agricultural growers and handlers in the U.S. The main concerns with such promotion programs are not necessarily related to the potential of such programs to increase overall demand. Indeed, past studies have shown that both commodity-specific and broad-based fruit and vegetable advertising have generally been effective in increasing consumption (see Alston *et al.*, 2005; Kaiser *et al.*, 2005 for a review of commodity-specific advertising and Pollard *et al.*, 2008; Capacci and Mazzocchi, 2011 and Rickard *et al.*, 2011 for broad-based advertising). Instead, potential industry participants raise concerns about the distributive implications across fruits and vegetables, uncertainty of returns, asymmetry in information about potential participants' returns, lack of government assistance and, generally, a reluctance to change the status quo in the industry.<sup>1</sup> Without arriving at a mutually agreeable funding mechanism, growers are left to continue with the system of commodity-specific, 'beggar-thy-neighbor' (Alston, Freebairn and James, 2001) promotion programs as growers of different fruits and vegetables advertise actively to take market share away from others. In this article, we attempt to explain why growers are often reluctant to

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<sup>1</sup> While there was never an actual vote on the proposed broad-based campaign among fruit and vegetable producers in the United States, an industry trade group, *Produce for Better Health* conducted a survey of fruit and vegetable growers in 2009 regarding a hypothetical mandatory broad-based promotion program for the industry. The results of the poll indicated that only 22% of fruit and vegetable growers were in favor of this program, while 31% were opposed and the largest group (47%) were undecided. Of those indicating they opposed the program, 23% said the main reason for their opposition was that 'not everybody will benefit equally/be charged equally' (Produce for Better Health, 2009).

fund generic broad-based commodity promotion programs even under a high likelihood of positive returns, using a public-good voting experiment.

Following a decade of research on generic commodity-specific advertising programs, the literature agrees that returns from current programs do not distribute equitably across producers. First, opponents argue that generic advertising can weaken producer attempts to differentiate or brand their products because generic advertising frames all products as homogeneous commodities. While generic advertising increases primary demand for a certain commodity, the concern is that it also redistributes "shares of the pie" inequitably among growers or producers. Second, small producers argue that the money contributed to generic advertising campaigns only help large players and provide little benefit to the rest of the industry. Both theoretical and empirical research support these concerns. Chung and Kaiser (2000) and Zheng, Bar and Kaiser (2010) found that inequality becomes a significant issue when industries have substantial differences in firm-level supply elasticities, firm sizes, and experience large demand shifts due to advertising programs. Similarly, Hamilton, Richards and Stiegert (2013) found that the likelihood beef producers favored expanding the "*Beef. It's What's for Dinner!*" campaign increased with their operating scale. Several empirical and lab experiment studies (e.g., Crespi and Marette, 2002; Chakravarti and Janiszewski, 2004), have also provided supporting evidence. Multi-commodity ('broad-based') generic advertising programs, which are the focus of this paper, are likely to be even more unequal than the single commodity cases.

At its core, a typical generic commodity promotion program is a public good (Alston *et al.*, 2005) because the benefits are arguably non-rival and non-exclusive and, therefore, subject to free-riding. Benefits to commodity promotion are non-rival because an increase in demand raises prices for all producers, while they are non-exclusive because it is impossible to prevent an

individual grower from taking advantage of higher prices.<sup>2</sup> Growers' reluctance to participate in a truly generic fruit and vegetable promotion program may be partially due to 'self-centered inequity aversion' (Fehr and Schmidt, 1999). Fehr and Schmidt (1999) argue that observed over-contribution to public goods in the lab is due to individuals' experiencing negative utility whenever they earn significantly more or less than others. Instead, we modify their behavioural postulate to explain under-contribution as resulting from 'self-interested inequity aversion.' Self-interested inequity aversion means that individuals experience negative utility when others benefit more from a public good than they do, but positive utility when they earn more than others. A person's decision to contribute to the public good is thus validated when the good provides advantageous returns to themselves relative to others.

Contributions to generic advertising programs are governed by institutions set up to enable collective marketing efforts; institutions which are, in turn, formed by members' voting behaviour. Institutional arrangements are well understood to influence contributions in a variety of social dilemma situations, including common property resources (Walker *et al.*, 2000), charitable giving (Croson, 2007), public goods (Kroll, Cherry and Shogren, 2007) and generic commodity advertising (Messer, Kaiser and Schulze, 2008). Kosfeld, Okada and Riedl (2009) focus on the endogenous formation of institutions that may compel agents to contribute based on some voting scheme. Equilibrium voting patterns in their model depend critically on the preferences of members and, indeed, show that the inequity aversion model of Fehr and Schmidt (1999) can generate a 'grand organization' in which all members of the industry agree to contribute. Voting decisions, therefore, are a realistic way to measure the willingness to participate, or to compel others to

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<sup>2</sup> Clearly, institutional mechanisms could be designed to minimize these two features, but we describe a typical commodity example.

participate, in any proposed broad-based advertising program.

We test our model of self-interested inequity aversion using a public-good voting experiment that broadly describes the broad-based fruit and vegetable advertising case. Subjects participate in several rounds of a market-equilibrium experiment in which advertising generates stochastic returns among participants. After participating in several simulated ‘years’ of commodity-specific advertising, they are asked whether they would like to participate in a mandatory broad-based advertising program. All subjects see both their own past returns and, in some treatments, others’ past returns from advertising. This is done to provide a context for the subjects similar to what most fruit and vegetable producers in the U.S. experience. Their vote is assumed to reflect the perceived utility derived from participating in the advertising program.

Our results show strong support for the self-interested inequity aversion hypothesis. We also find uncertainty of returns to be a significant variable in explaining voting behaviour, and that subjects are risk averse when they have relatively advantageous returns, but are risk loving in the opposite case. Among treatments designed to control for non-stochastic returns, partial information, government subsidization of advertising and experience with program effect, we find that if returns are equal across producers, if there is government support for the program or if the information about others’ payoffs is incomplete, the likelihood of approval for broad-based advertising increases. Lastly, we find that if subjects are allowed to experience several periods under a mandatory generic broad-advertising program before they vote for it—a procedure commonly used by agricultural commodity organizations to gauge producer interest—the probability of voting to keep the program increases.

We contribute to the generic advertising, and more broadly to the public goods, literature in two ways. First, it is well understood that generic advertising has many features of a classic public

good (Alston *et al.*, 2005). However, there is little research examining the failure of promotion referenda or the many legal challenges to the system of generic commodity advertising (Crespi, 2003) using behavioural models of public good contribution. Second, we provide experimental evidence on the importance of inequity aversion relative to risk aversion, or efficiency, in determining whether a public good will be funded.

The paper is structured as follows. In the next section, we provide some background on voluntary contribution mechanisms and behavioural explanations for under-contribution to public goods. Next, we describe a simple theoretical model of inequity aversion, and derive testable, competing hypotheses regarding the likely effect of unequal returns on voting behaviour. In the subsequent section we describe the experimental procedures used to generate the data and test our hypotheses. The fifth section describes how the data were analysed and explains and interprets the results obtained. The last section summarizes our research and presents its implications.

## **2. Background and relevant literature**

In the absence of traditional mandatory-funding systems, voluntary funding mechanisms appear to be the most viable means of sustaining promotion programs that benefit large groups of agricultural producers. Messer, Schmit and Kaiser (2005) study one voluntary contribution mechanism – the provision point mechanism– in which a program is funded only if a certain proportion of growers agree to support it, at which point it becomes mandatory for all. They find an optimal range for the threshold provision point (that maximizes producer surplus) to be between 68% and 90%, depending upon the effectiveness of promotion. Although designing an alternative contribution mechanism may provide a means of funding generic advertising program, it does not address the underlying cause of the problem: an observed reluctance to contribute, despite the fact that returns

tend to be relatively high (Alston *et al.*, 2005). Because returns to most programs are generally high, we look for a behavioural cause.

Researchers have long observed, both in the lab and under real-world situations, that contributions to linear public goods do not match what conventional theory would predict. In a basic linear public goods game, subjects privately choose how much to put towards a public good. The total amount of contribution increases linearly with the number of participants and is divided evenly among players. The group's total payoff is maximized when everyone contributes everything to the public pool. However, the analytical Nash equilibrium of this game is zero contributions by all. Contrary to the theoretical predictions, observed contributions are significantly higher in the real world (Rabin, 1993; Fehr and Schmidt, 1999; Fehr and Gächter, 2000; Bolton and Ockenfels, 2000). Behavioural research addresses both under-contribution and over-contribution to linear public goods. Rabin (1993) argues that over-contribution need not be altruistic by recognizing that individuals tend to help those who can help advance their own interests, and hurt those who can harm their own interests. Fehr and Gächter (2000) test the hypothesis that individual agents prefer not to be the 'sucker' in public good games. That is, given the opportunity to punish free-riders, they will do so even if such punishment is costly relative to the benefits expected from future rounds of the game. Bolton and Ockenfels (2000) expand the concept of self-interest to allow considerations of equity, reciprocity and competition (ERC) to influence an individual's incentives to give.<sup>3</sup> Kritikos and Bolle (2001), however, find that the model of inequity aversion developed by Fehr and Schmidt (1999) provides a better fit to experimental data than the ERC model.

Fehr and Schmidt's (1999) model of self-centred inequity aversion assumes individuals

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<sup>3</sup> Reciprocity in public good games refers to the notion that individuals will give at least as much as the minimum contribution of others so that, in some sense, they obtain utility from contributing as much as anyone else (Shang and Croson, 2009).



experience negative utility whenever they earn more or less than others from the public good. Heterogeneity among agents in their aversion to inequity is important, however, as they show that just a few selfish individuals can cause contributions to fall to zero, while only a few inequity-averse players can cause contributions to rise well above the competitive level. Inequity aversion is self-centred in the sense that ‘...people do not care per se about inequity that exists among other people but are only interested in the fairness of their own material payoff relative to the payoff of others’ (Fehr and Schmidt, 1999: 819). Redistributive contributions result from individual incentives to correct the inequity inherent in benefits from the public good.

Finding that inequity increases contributions, however, is not universal. Binmore and Shaked (2010) and Binmore (2011) present a wide-ranging critique of the inequity aversion theory and the subsequent literature applying the model and documenting the phenomenon. There have also been a number of studies and applications questioning the adequacy of Fehr and Schmidt’s data or the range of application and showing that the predictions of the model do not hold (e.g., Charness and Rabin, 2002 and Engelmann and Strobel, 2004). Inequity, or heterogeneity in the benefits derived from public goods among agents more generally may, in fact, reduce contributions (Ledyard, 1995). Cherry, Kroll and Shogren (2005) design an experiment in which they test subjects’ willingness to contribute in a simple linear, public-good framework in which both initial endowments are allowed to differ. They find that contribution amounts are significantly lower when endowments are allowed to vary among subjects relative to when the distribution is homogeneous. The origin of the heterogeneity – whether the differences were earned or not – did not matter. Kroll, Cherry and Shogren (2007) investigate whether their previous finding (Cherry, Kroll and Shogren, 2005) that heterogeneity in endowments causes contributions to fall – due to what they term ‘anticipatory reciprocity’ – and that the source of endowments does not matter are robust to the

nature of the public good game. Specifically, they consider best-shot games in which the amount of the public good is determined by the largest contribution and not the sum of everyone's contribution. They find that heterogeneity is again important, and that the source of the endowment matters in these asymmetric games.<sup>4</sup>

In the following analysis, we utilize a public-goods voting experiment in an attempt to explain the reasons why growers are reluctant to fund a generic broad-based commodity promotion program for fruits and vegetables in the United States. Consistent with Fehr and Schmidt (1999) and observations in actual commodity markets, our theory focuses on heterogeneity in returns as a source of inequity.

### **3. Theory and hypotheses**

We extend the model of fairness in Fehr and Schmidt (1999) to build a predictive model of the probability that producers will contribute to a broad-based generic promotion campaign. Intuitively, our model suggests that the greater the inequity in returns among producers, the less likely a particular producer is to vote in favour of the checkoff program. Based on the general principle of inequity aversion, we test two hypotheses: (1) self-centred inequity aversion in which agents experience negative marginal utility when they earn more or less than others from the public good, and (2) self-interested inequity aversion in which agents experience negative marginal utility only if they earn less than others, but positive utility if they earn more than others. According to Fehr and Schmidt (1999), the degree of self-centred inequity is predicted to rise in the degree of

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<sup>4</sup> Recently there have been numerous studies that have focused on various other settings of inequity that merit a mention, such as heterogeneous marginal returns from public goods (e.g., Reuben and Riedl, 2013), marginal impacts to the public goods (e.g., Fisher *et al.*, 1995; Noussair and Tan, 2009), or effects of the conflicting contribution norms of players in different roles (Bernard *et al.*, 2011; Reuben and Riedl, 2013).

disadvantageous inequity (the amount others make more than the producer) at a greater rate than with respect to advantageous inequity (the excess returns to the producer relative to others). In our extension to their model, the effects of disadvantageous and advantageous inequity are expected to differ in sign.

An aversion to risk may also explain an agent's unwillingness to participate in the promotion campaign. Because returns earned by subjects in our experiment are subject to varying degrees of risk, we can estimate the effect of risk aversion on the probability of contribution using a Mean-Standard Deviation utility-function approach. Meyer (1987) shows that a utility function that is linear in the mean and standard deviation of returns will produce outcome rankings that are consistent with those derived from an expected utility framework and, more importantly, can be used to test similar hypotheses regarding the nature of risk-averse behaviour among respondents.

Formally, the indirect utility function that reflects this logic is given by:

$$V_h(r_h, r_{-h}) = r_h - \alpha_h DI_h - \beta_h AI_h - \theta_h \sigma_h + \varepsilon_h, \quad (1)$$

where:

$$DI_h = \left( \frac{1}{n-1} \right) \sum_{h \neq -h} \max(r_{-h} - r_h, 0), \quad (2)$$

is the measure of disadvantageous inequity, and:

$$AI_h = \left( \frac{1}{n-1} \right) \sum_{h \neq -h} \max(r_h - r_{-h}, 0), \quad (3)$$

measures advantageous inequity, where  $r_h$  are the returns to agent  $h$  from participating in the checkoff program,  $r_{-h}$  are the returns to other agents,  $\alpha_h$  is a measure of the marginal disutility from disadvantageous inequity (others earn more than agent),  $\beta_h$  is a measure of the marginal disutility from advantageous inequity (others earn less than agent  $h$ ), and  $\theta_h$  is the marginal

disutility of risk ( $\sigma_h$  is the standard deviation of returns experienced throughout the experiment up to the vote by each subject). We test hypotheses regarding the effect of each type of inequity on voting behaviour in a checkoff referendum by adding a vector of observed demographic features to equation (1), and include a measure of unobserved heterogeneity among experiment subjects,  $\varepsilon_h$ .

Risk aversion cannot be measured by the estimate of  $\theta_h$  alone as the concept of risk aversion refers to the trade-off between risk and returns. Meyer (1987) shows that the degree of risk aversion can be measured by the slope of the indifference curve between risk and return, which in our case is written as:

$$S^{AI}(\sigma_h, AI_h, DI_h) = \frac{-V_{\sigma}(\sigma_h, AI_h, DI_h)}{V_{AI}(\sigma_h, AI_h, DI_h)}, \quad (4)$$

for advantageous inequity, and similarly for the slope of the indifference curve with respect to  $DI_h$ , where  $V$  subscripts indicate partial differentiation. If  $S^{AI} > 0$  then equation (4) implies risk aversion,  $S^{AI} = 0$  implies risk neutrality, and  $S^{AI} < 0$  risk preference (Eggert and Tveteras, 2004). Although our linearity assumption implies that the slope of  $S^{AI}$  in the amount of advantageous inequity, or the concavity of the indifference curve, is zero by construction, we are able to test and identify how risk preferences change in the amount of  $AI_h$  or  $DI_h$  through a random-parameter estimation framework by constructing the experiment such that returns vary over individuals with varying preferences toward risk.<sup>5</sup>

Assuming  $\varepsilon_h$  is Type I Extreme Value Distributed and allowing for  $\alpha_h$ ,  $\beta_h$ , and  $\theta_h$  to each vary among agents, we estimate a random coefficient logit model of voting behaviour where the probability of voting in favour of the checkoff program is given by:

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<sup>5</sup> Note that we normalize returns to 1 in the indirect utility function, so each of the risk-preference parameters refers specifically to risk behavior with respect to AI, or DI, whichever the case may be. The notion of returns that is relevant to decision making in this model is returns relative to other agents' returns.

$$\Pr(y = 1) = \frac{\exp(r_h - \sum_i \gamma_i x_{hi} - \alpha_h DI_h - \beta_h AI_h - \theta_h \sigma_h)}{1 + \exp(r_h - \sum_i \gamma_i x_{hi} - \alpha_h DI_h - \beta_h AI_h - \theta_h \sigma_h)} \quad (5)$$

where  $y$  is a binary variable that equals 1 when the subject votes for the checkoff and 0 when the subject votes against it, and  $x_{hi}$  is a vector of demographic variables describing agent  $h$  and controls for other treatments such as stochastic returns, partial information, government support, and prior experience with the program.

With this model, the self-centred inequity hypothesis, conditional on the degree of risk aversion, is given by  $H_0: \alpha_h = \beta_h = 0$  against the alternative hypothesis:  $H_a: \alpha_h > \beta_h > 0$ , or that there is indeed a significant disutility of inequity in returns to the checkoff program, and that the marginal disutility of returns is greater for disadvantageous inequity than for advantageous inequity. Therefore, the greater the difference in returns across agents, the less likely a subject is to vote in favour of the checkoff program. Self-interested inequity aversion, however, is framed in terms of the joint hypothesis:  $H_0: \alpha_h = \beta_h = 0$  against a different alternative hypothesis:  $H_a: \alpha_h > 0; \beta_h < 0$ . In this case, the agent is less likely to vote for the program if others earn more, but is more likely to vote for the program if the agent earns more than others. The greater the difference in disadvantageous returns among agents, the less likely a subject is to vote in favour of the checkoff program, but the greater the advantageous returns, the more likely to vote in favour.

Testing for the effect of inequity on the likelihood of supporting the program requires that we control for other potential explanations for why producers may be unwilling to contribute to generic advertising programs besides risk aversion. In our economic experiment, we include treatments for a number of alternatives. Returns to advertising in the ‘real world’ vary among producers due to differences in production technology, managerial skill, or any one of a number of producer-specific attributes. Therefore, our control scenario allows for both unequal and stochastic

returns in order to mimic actual practice as nearly as possible. Unlike the control group, the returns in the equity treatment (Treatment 2) are designed to be equal across all commodity groups. We also include four extension treatments to track how various factors might affect the probability of a favourable vote. All of the treatments are described in detail below.

## **4. Methodology and experimental design**

### *4.1 Experiment design*

All experimental sessions were conducted in the Cornell Lab for Experimental Economics and Decision Research. A total 285 subjects participated in the experiment. All sessions lasted just under one hour and subjects earned on average \$25 with payouts ranging from \$17 to \$30; payments were related to subjects' performance in the competitive experiment and on the quiz testing subjects' comprehension of the referendum question. Six different treatments were designed to test the theory and hypotheses developed in the previous section and are summarized in Table 1.

Subjects were seated randomly at individual computer terminals with privacy shields, and were informed that all decisions they made would be kept strictly confidential. Each session included 22 to 24 subjects. We ran two sessions per treatment, which resulted in 45 to 48 total participants per treatment. After signing a consent form, participants were given a brief introduction on the experiment, which included the basic rules of the experiment, and were invited to start reading the instructions to Part A. Participants were informed that, depending on their random seating assignment, they belonged to one of the three producer groups. Each producer group was assigned one of three fictitious commodities.

The experiment consisted of three parts (Part A, Part B, and Part C) designed to emulate the same order of the history of funding for fruit and vegetable generic advertising in the United States.

Subjects were not aware of the specific instructions for each part as separate instructions were distributed prior to the start of each part. Part A of the experiment did not involve an advertising program. Part B included a mandatory commodity-specific generic advertising program for the three individual commodities. Part C was the same as Part B, except that subjects were asked to vote on an additional mandatory broad-based generic advertising joint for all three commodities based on treatment specific variation in information that was presented to subjects. After the vote, another four rounds of bidding were implemented. If the broad-based program approval was higher than or equal to  $2/3$  of the total votes, we added broad-based advertising to Part C bidding rounds. Conversely, if the vote was below  $2/3$ , we implemented rounds only with commodity-specific advertising. Figure 1 illustrates the timeline of the experiment.

**4.1.1 Part A – No generic advertising program.** The experimental design in this part of the experiment is similar to that used by Messer *et al.* (2005, 2008). To familiarize subjects with the experiment's procedures, the first part of the experiment consisted of four rounds ('years') of bidding and did not include the advertising program. Subjects received written instructions (see Appendix A), and the administrator provided a verbal description of the experiment and answered any questions. Subjects were randomly assigned to a computer that had a spreadsheet informing them of their costs for selling five units of the commodity. Each subject's costs varied over each round, and over all four rounds subjects were exposed to the same distribution of unit costs. We did this to make the experiment as fair as possible for all subjects since they were competing against each other to earn money. The unit costs differed for each subject and each unit ranging from \$1.00 to \$2.75 (depending on the part of the experiment); subjects only incurred the cost if the unit was sold. Each of the three commodity groups of eight subjects had the same unit cost structure.

Using Excel spreadsheets programmed with Visual Basic for Applications, subjects submitted their offers to sell their units to the experiment administrator. These offers were stored in a database.<sup>6</sup> The administrative computer calculated the market price and the number of units sold by each subject. This information was also stored in the database. When notified by the administrator, the subjects retrieved this information and profits were calculated after each round.

The administrator acted as the buyer in the market, where demand was stochastic, ranging from 11 to 29 units per commodity. For each round and for each of the three commodity groups, demand was determined randomly by a random number generator. A triangle distribution was used where the average demand (20 units) had a 10% chance of occurring, while the extremes (11 units and 30 units) had a 1% chance of occurring. The use of stochastic demand resulted in price fluctuations that are similar to price fluctuations observed in commodity markets. That is, the amount of variation in prices in all sessions across all rounds, as measured by the coefficient of variation (CV), was 11.1%, which is fairly close to the CV of 13.6% computed for the ratio of the index of prices received to prices paid by farmers from 1980-2012 (Economic Report of the President, 2013). Similar to Messer *et al.* (2005) and Messer *et al.* (2008), demand in the experiment was assumed to be perfectly price inelastic<sup>7</sup> and the supply elasticity was set at 1.5. Based on these parameters and an expected demand of 20 units, the average equilibrium price for the experiment was \$1.50 per unit (see Appendix A for specific details on the probabilistic

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<sup>6</sup> Following Messer *et al.* (2008) and Messer *et al.* (2005), subjects were reminded in their written instructions that the market was competitive and therefore, submitting offers that are too high might result in forgone profitable trades. Previous researchers such as Messer *et al.* (2008) have used this approach to speed up the natural outcome in bidding behavior since most subjects eventually learn to submit bids equal to their costs in repeated round offers with this type of auction.

<sup>7</sup> Similar to Messer *et al.* (2005) and Messer *et al.* (2008), demand in the experiment was assumed to be perfectly price inelastic and the supply elasticity was set at 1.5. The perfectly price inelastic demand was assumed so that the stochastic nature of demand was transparent to subjects. Given previous estimates of demand elasticities, (e.g., fluid milk, -0.04 (Schmit and Kaiser, 2004); eggs, -0.02 to -0.17 (Brown and Schroeder, 1990); walnuts, -0.08 (Kaiser, 2002); almonds, -0.20 (Crespi and Chacon-Cascante, 2004); and pork, -0.20 (Reed, Levedahl, and Clark, 2003) this assumption appears to be plausible.



distribution of demand).

A uniform price auction determined the market price for each round and each commodity group by setting the price for all units sold at the *first rejected offer*. The uniform price auction is common in experimental settings because of its incentive-compatible characteristics, transparency, and ease of administration (Cox, Smith and Walker, 1985; Messer *et al.*, 2009). To illustrate, suppose random demand is drawn to be 20 units. Then the market price for all units sold would be set by the offer for the 21<sup>st</sup> unit.

**4.1.2 Part B – Mandatory generic commodity-specific advertising.** To imitate the history of most generic commodity-specific advertising programs, the second part of the experiment incorporated a mandatory commodity-specific, advertising program for each of the three commodities. Like Part A, Part B consisted of four rounds, but sellers were now informed that their unit costs increased by \$0.25 and the proceeds were used to fund a separate advertising program for each of the three commodity groups. Subjects were informed that past research has found that commodity-specific advertising has had a significant positive effect on the market price and producer profits. Sellers were also told that advertising would deterministically increase each of the three commodities' stochastic demand by three units. For example, if stochastic demand were drawn to be 20 units, an additional three units would be automatically added to this due to advertising and hence market demand would be 23 units. The assessment rate and corresponding increase in expected price was set to parallel the relatively high return on investment, roughly three-to-one, frequently found with generic commodity advertising (Alston *et al.*, 2005; Kaiser *et al.*, 2005).

**4.1.3 Part C1 – Vote: mandatory broad-based advertising program or no program.** All subjects

went through the same procedures in Parts A and B to provide context similar to what most fruit and vegetable growers experienced with and without commodity-specific advertising. In Part C1, subjects were asked to vote on a referendum for an additional \$0.15 mandatory assessment for a broad-based advertising campaign under six different treatments. Similar to actual checkoff program referenda, subjects were told that if at least two-thirds of subjects voted for the broad-based program, then all subjects, including those who did not vote for it, would have to contribute the \$0.15 assessment on each unit sold, which was on top of the \$0.25 assessment for the commodity-specific program. The difference in commodity-specific and broad-based advertising was explained to all subjects.

**4.1.4 Part C2 - Comprehension test.** Prior to administering the vote for broad-based advertising, we provided a monetary incentive for subjects to fully understand what they were voting on in the referendum. Subjects were asked to carefully read and later listen closely to an explanation of what the broad-based program entailed; they would then answer four test questions prior to voting, and would be awarded additional compensation if they answered all questions correctly the first time. If they answered at least one wrong the first time, but all correctly the second time, they received a smaller level of additional compensation. Finally, they received no additional money if they did not answer all questions correctly in the first two attempts, but were still required to retake the test until they got all questions correct.

**4.1.5 Part C3 - Mandatory broad-based advertising added or no broad-based advertising added.**

Once the subjects completed the comprehension test and voted, the experiment administrator calculated the votes and announced whether the broad-based advertising program would be

implemented. Depending on the vote results, the next four rounds contained either bidding with broad-based advertising program implemented or repeated the four rounds of Part B without broad-based advertising.

During all parts of the experiment we displayed the equilibrium market price for all rounds up to the current one. Subjects were instructed to look at the graphs after every round to see how market prices fluctuated with and without commodity-specific or broad-based advertising.

**4.1.6 Survey.** After the auctions and the referendum were completed, participants were asked to fill out a computerized survey regarding some characteristics thought to influence their vote. The survey questions are included in Table 2 and included subjects' opinions on the fairness, perceived profitability, and degree of information on impacts of the broad-based program. In addition, the questions included subjects' opinions on collective action, the involvement of government in the private sector, and general thoughts on marketing activities that are conducted by individual firms.

## *4.2 Main treatments*

**4.2.1 Treatment 1:inequity control group.** Treatment 1 was the control group and was based on factors that we hypothesize to have contributed to the low support for a mandatory broad-based program by the fruit and vegetable industry. The purpose of Treatment 1 was to reflect the current state of the industry. In this treatment, subjects were informed that the increase in demand due to broad-based advertising was stochastic with full probabilistic information given for all three commodities, that the expected impact was different for each of the three commodities, and that there was a chance that there will be no advertising-induced demand impact for each commodity. In this control treatment subjects were given all the information in Table 3 including the probabilistic information for the two other groups as well as their own group. As shown in Table 3, the \$0.15 assessment had a largely unequal expected demand impact: the expected increase in demand was one unit for Commodity Group 1, two units for Commodity 2, and three units for Commodity 3. Subjects were also informed that the increase in demand due to broad-based advertising was stochastic.

**4.2.2 Treatment 2:equity treatment group.** Treatment 2 relaxed the unequal returns condition of control treatment. Specifically, in this treatment, subjects were informed that, on average, the expected return from mandatory broad-based advertising would increase demand by two units for all three commodity groups.

## *4.3 Additional treatments*

Four additional treatments were designed as extensions to the main treatments by including additional factors that might affect the probability of a favourable vote.

**4.3.1 Treatment 3: inequity + non-stochastic returns.** With stochastic returns, inequity becomes perceived inequity as a difference in returns observed in the current period may reflect either structurally higher returns to advertising by another producer, or simply a random draw. We controlled for uncertainty in Treatment 3 with returns that were certain. With this treatment we intended to test whether uncertainty over the expected demand and hence price impact together with inequality in returns may be an impediment for supporting the program. Specifically, in this treatment, subjects were informed that demand due to mandatory broad-based advertising was guaranteed to increase by one unit for Commodity Group 1, by two units for Commodity Group 2, and by three units for Commodity Group 3. Since the rest of the five treatments all have an uncertainty element in them by design, we also control for participants' risk aversion by including measures of variability in experienced returns.

**4.3.2 Treatment 4: inequity + partial information.** The differences in perceived returns among producers may be due to the quality of information available. Producers typically do not have complete information on expected impacts for their own and alternative commodities, and inclusion of perfect probabilistic information allows testing the impact of such information on the vote. Treatment 4 was designed to relax the full information assumption. In this treatment, subjects were only given probabilistic information on the potential demand impacts for their own commodity, and no information on the other two commodities.<sup>8</sup>

**4.3.3 Treatment 5: inequity + government subsidy.** With Treatment 5 we tested the possibility that

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<sup>8</sup> Note that, because returns are stochastic, information is always incomplete (other than in Treatment 3, non-stochastic returns), but producers have full information about competitors' returns in all treatments except Treatment 4.

growers might expect an outside agent (the government) to address any perceived inequity by allowing for government support of the advertising program. The treatment is motivated by the real world observation that generic broad-based promotion programs for fruits and vegetables in other countries have received public support. It is expected that government support will reduce any perceived inequity in the system and, thereby, increase the likelihood of support. During this treatment, the subjects were informed that if the vote passed, the government would match the \$0.15 assessment and that this match would double the expected impact in Table 3.

**4.3.4 Treatment 6: inequity + experience with program prior to vote.** A more recent approach that the government and commodity organizations have used is to implement the mandatory checkoff program prior to holding the referendum for a trial period, e.g., using a trial period of 18 months. The idea here is that producers in the industry will be better informed about whether the program is effective as they experience it for a period of time.<sup>9</sup> In Treatment 6 we held four additional periods with broad-based advertising that none of the other treatments had prior to the vote. Unlike the other treatments where participants were asked to vote to opt-in a broad-based advertising program, in this treatment they automatically participated in four rounds of the broad-based advertising program so they had to experience the returns from this program before voting. Once the four rounds were over, we asked them to vote whether to keep or to discontinue the program. Treatment 6 was designed to test whether experiencing the program prior to the vote had a positive impact on the vote.

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<sup>9</sup> The checkoff program for cut flowers, which was a mandatory federal program followed this approach

## 5. Estimation procedure

Producer heterogeneity is incorporated into the voting model (2) by allowing the marginal disutility of inequity (both advantageous and disadvantageous) to vary randomly by subject. This assumption is necessary because there are many unobservable factors associated with individual subjects that may be relevant to how they let inequity affect their voting behaviour. Random  $c$  can be decomposed into three components: 1) mean levels of inequity and risk measures ( $\alpha_0$ ,  $\beta_0$ , and  $\theta_0$ ); 2) component that varies with expected returns ( $\alpha_1$ ,  $\beta_1$ , and  $\theta_1$ ) and 3) component capturing unobserved inequity and risk preferences:

$$\alpha_h = \alpha_0 + \alpha_1 E(\pi_h) + \varphi_\alpha v_h, \quad v_h \sim N(0,1), \quad (6)$$

$$\beta_h = \beta_0 + \beta_1 E(\pi_h) + \varphi_\beta v_h, \quad v_h \sim N(0,1), \quad (7)$$

$$\theta_h = \theta_0 + \theta_1 E(\pi_h) + \varphi_\theta v_h, \quad v_h \sim N(0,1), \quad (8)$$

where  $v_h$  is unobserved agent  $h$  heterogeneity and  $\varphi$  parameters capture how  $\alpha_h$ ,  $\beta_h$  and  $\theta_h$  vary with individual preferences and can be interpreted as standard deviations of these inequity and risk measures.<sup>10</sup>

We measure the returns to each subject by taking the expected returns over the experiment. We do so in order to capture the profit the subject can expect to receive should the mandatory program come into effect. Expected profit also captures the observation that perceptions of inequity are relative to what each agent expected to receive (Fehr and Schmidt 1999). Defining inequity in relative terms is necessary in order to capture the behaviour described by Bolton and Ockenfels (2000), namely that subjects are motivated not by absolute returns, but by what they earn

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<sup>10</sup> We allow the random coefficients to follow standard normal distributions in order to impose as few restrictions on the empirical model as possible. Because our theory imposes no restrictions on the unobserved heterogeneity that may be present in these estimates, we are reluctant to do so in the empirical model.

relative to expectations of their own returns and the returns earned by others. This specification for the random coefficients also allows us to examine how the degree of risk aversion changes in the level of expected returns. To this end, we designed the experiment to replicate real-world conditions as nearly as possible by allowing for stochastic returns in all but one treatment. With stochastic returns, players must form expectations about both their own and others' returns in deciding whether to contribute to a generic advertising program.

We estimate the model in (5) using simulated maximum likelihood. Simulated maximum likelihood is necessary because a closed form solution for the probability in (5) does not exist when integrating over the distributions of unobserved heterogeneity for the marginal utility of disadvantageous inequity, advantageous inequity, and risk. We test our random coefficient assumption in the next section against a simpler, more parsimonious fixed-coefficient logit alternative.

## **6. Empirical results**

The descriptive statistics for the data collected in the experiment are displayed in Table 4. In the base scenario, only 45.8% of subjects voted in favour of the proposed mandatory broad-based program. Table 4 also displays the outcome of the vote for the broad-based advertising program for the other treatments. As expected, the control scenario (Treatment 1) resulted in the lowest approval of the program. The proportion of favourable votes was statistically significantly different from the control for equal returns, government support and experience with the program treatments at 95% confidence interval or above. Demographic composition and academic performance were very similar across all treatments. In Table 4 we report whether there were any statistical differences in these variables among treatments by testing whether each mean within a demographic category was



statistically different from the overall mean. We found that the vast majority of demographic and academic performance means were similar across treatments. Small differences in means are not a concern in our model, since we include controls for these variables in our econometric model. The proportion of the participants who expressed their vote but were in fact uncertain about their decision was the lowest in the control treatment (16.7%) and the highest in the equal returns treatment (31.3%); approximately a quarter of participants were uncertain about the vote in the rest of the treatments. Finally, the answers to questions asking participants whether they strongly disagree (1) or strongly agree (9) with statements indicated that on average in all treatments, subjects had generally mixed opinions, as mean responses to all questions were around 5. The following discussion of results is based on the estimation results in order to control for possible differences in subjects across the various treatments.

Table 5 presents the results obtained by estimating the logit model in (5), both the fixed-coefficient logit and random-coefficient logit versions<sup>11</sup>. Comparing the log-likelihood function (LLF) values of the two models using a likelihood ratio (LR) test, we easily reject the fixed-coefficient in favour of the random-coefficient logit model. Clearly, unobserved heterogeneity is an important problem in estimating voting behaviour on whether to fund generic advertising programs. Because our data support the random-coefficient model relative to the fixed-coefficient logit model, we use the former to test our hypotheses regarding inequity aversion. Several non-treatment variables were statistically significant in the random-coefficient model. Males had a higher probability of voting for the program. Subjects holding libertarian views on choice were less likely to vote for the program, whereas subjects who believed that collective action to improve

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<sup>11</sup> The model was estimated using the statistical package NLogit (version 5.0). A random coefficients model was estimated with 50 Halton draws and the reported results were found to be robust to alternative numbers of draws.

overall industry profits is good were more likely to vote positively.

Recall our two hypotheses above: (1) self-centred inequity aversion and (2) self-interested inequity aversion. From the results in Table 5, we find that we cannot reject the null hypothesis of self-interested inequity aversion as  $\alpha_h > 0$  and  $\beta_h < 0$  and reject the hypothesis of self-centred inequity aversion. Therefore, the primary hypothesis of Fehr and Schmidt (1999) is not nearly as other-regarding in our context as experiment subjects only experience disutility of inequity if it is not in their favour, but prefer inequity when it is in their favour. Compared to the notion of reciprocity advanced by Bolton and Ockenfels (2000) and Shang and Croson (2009), our finding suggests exactly the opposite: that individuals only contribute to the extent that they perceive themselves above others, and not below. In this regard, our result is more akin to Rabin (1993) and Fehr and Gächter (2000) in that individuals contribute only if they have a vested self-interest in doing so, and there is little likelihood that they will be the ‘sucker’ who receives less than one’s fair share in return for contributing.

We can use the estimates of  $\theta_h$  to infer the degree of risk aversion among our sample subjects both at the mean of the data, and as a function of expected returns. The value of  $S^{DI}$  implied by these estimates is -0.057, or a slight affinity for risk when others earn more than subject  $h$ . The average subject is slightly risk averse ( $S^{AI} = 0.103$ ) when earning more than others. This finding is intuitive – when at an advantage, we would expect an agent to be particularly sensitive to the variability of returns, but when making less than others, the agent may indeed prefer a better draw to catch up. Both of these effects are attenuated as expected returns increase. That is, as returns rise, agents at an advantage will become less risk averse and those at a disadvantage relative to others will become less risk-loving. Both of these interpretations are again to be expected given what we know about loss aversion from Kahneman, Knetsch and Thaler (1991) and others.

In the main treatment, Treatment 2, we test for the effect of allowing returns to be equal rather than unequal. This treatment fixed effect is significant at the 1% level. If expected returns are equal, *ceteris paribus*, then subjects are more likely to vote for the broad-based program. While the direct implication of this result is intuitive (given that mean returns are positive in the experiment design), it also supports our hypothesis that self-interested inequity aversion is conditional on the possibility that mean-returns can be equal. Even if inequity is observed as the result of a random draw, therefore, producers will be more likely to support the program if they benefit more than others.

The results presented in Table 5 allow us to test the extensions considered in the four additional treatments that focus on other factors of perceived inequity. Subjects in Treatment 3 were not found to be more or less likely to support the referendum than in the control treatment. Recall that returns in this treatment were non-stochastic, while returns in the rest of the experiment were subject to a random draw from a known probability distribution. The finding that the Treatment 3 fixed effect is statistically insignificant is not surprising; in our estimation we already control for risk aversion across all treatments. The significant risk aversion parameter suggests that certainty in returns increases the likelihood of participation.<sup>12</sup> Although the effect of Treatment 3 is not significant at a 10% level, the positive point estimate is important, and to be expected given previous research on individuals' abilities to make decisions under uncertainty. Most notably, Tversky and Kahneman (1971) describe a specific violation of the axiom of expected utility maximization. They find that subjects tend to adhere to the 'law of small numbers', or the incorrect belief that the probability distribution of the mean from a small sample of random values is the

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<sup>12</sup> Alternative models were estimated in which we included various measures of each subject's history of returns in each part. None of these specifications proved to be statistically significant so were not included in the final model.

same as the distribution of the mean from a large sample. In our experiment, the law of small numbers means that subjects may incorrectly assume that the mean of their small sample is the same as the expected value of the random variable itself, or ‘over-infer’ from their experience in a manner similar to mutual fund investors chasing a high-performing manager, or a roulette player following ‘hot’ numbers (Kahneman and Smith, 2002).

We also test for the effect of removing our assumption of full information (Treatment 4). Partial information means that participants do not see the distribution of likely returns as a result of the broad-based advertising program, and only have probabilistic information on their own returns. We find that, controlling for risk aversion, partial information increases the likelihood of broad-based advertising support. This result can be attributed to the fact that the truncated information that was provided to participants did not include any indications that returns might not be equal across the commodity groups. We know from the Treatment 2 results that equal returns were an important and significant driver for program approval. Therefore, lack of information about the competitors’ returns and lack of indication that those returns might be unequal affected the likelihood of support positively.

Next, we examine whether government support of the program in the form of a match to the advertising fund would have a positive impact on the vote for the broad-based program. As expected, we find that the government support treatment has a positive impact on the vote, and it is statistically significant. Everything else constant, a matching government subsidy effectively increases the benefit/cost ratio for producers so it is not surprising that such government support has a positive impact on making the broad-based advertising program more attractive to producers.

Finally, we find that Treatment 6 (having experience with the program prior to the vote) has a significant effect on voting behaviour. If participants have experience with broad-based

advertising and are asked to opt-out rather than opt-in, then the likelihood of support is higher. Our result is very similar to result by Cherry, Kallbekken and Kroll (2011), where subjects voted more often in favour of an efficiency-increasing Pigouvian tax after they had experience with it (and with the higher earnings it generated) compared to when they saw it on the ballot for the first time.

## **6. Summary and industry implications**

In this research, we examined several possibilities as to why growers are reluctant to fund generic broad-based commodity promotion programs even under a high likelihood of positive returns. Our analysis focuses on one possibility for this behaviour that we refer to as ‘self-interested inequity aversion’, which holds that individuals experience negative utility when others benefit *more* from a public good than they do, but positive utility when they earn more than others. This behavioural postulate is an asymmetric modification of Fehr and Schmidt’s (1999) model of ‘self-centred inequity aversion’ used to explain producer reluctance to fund broad-based advertising.

We test a model of self-interested inequity aversion using a public-good voting experiment that emulates the broad-based fruit and vegetable advertising case. The subject pool consisted of 285 participants who participated in several ‘years’ of a market-equilibrium experiment in which broad-based advertising generated stochastic and unequal returns among participants. At the end of each experiment, subjects were asked to vote on a mandatory broad-based advertising program.

Our results show strong support for the self-interested inequity aversion theory. We also find that uncertainty of returns explains voting behaviour: the higher the variability in experienced returns, the lower the probability of a favourable vote. Because we made a number of assumptions in framing our public good experiment, subjects were assigned to several treatments designed to

control for equal returns, non-stochastic returns, partial information, government subsidization of the public good and experience with program effect. Among these treatments, we find that if returns are equal across producers, the likelihood of a favourable vote rises. Additionally, we find that government subsidy and lack of information about competitors' payoffs increases the likelihood of broad-based advertising support. Finally, we find that if subjects are allowed to experience several periods of positive returns under a generic advertising program, they will be more likely to vote in favour of keeping the program. The favourable vote proportions are similar for government subsidy and experience treatments. Hence, the important barriers to implementation of a mandatory fruit and vegetable broad-based advertising program include grower uncertainty over expected returns from the program, perceived inequities across commodities, lack of government support and a bias stemming from not experiencing such a checkoff program.

## Under-contribution to generic advertising due to self-interested inequity aversion

**Table 1. Experiment Design**

	Treatments						
Attributes:	T1	T2	Extensions	T3	T4	T5	T6
Equal Returns <sup>1</sup>	No	Yes		No	No	No	No
Certain Returns <sup>2</sup>	No	No		Yes	No	No	No
Incomplete Information <sup>3</sup>	No	No		No	Yes	No	No
Government Support <sup>4</sup>	No	No		No	No	Yes	No
Experience with Program <sup>5</sup>	No	No		No	No	No	Yes

Notes: <sup>1</sup> *Equal Returns* implies that expected returns across 3 groups are the same and equal to group 2 returns as described in Table 3, whereas the rest of the treatments have unequal returns as described in Table 3; <sup>2</sup> *Certain Returns* implies that there is a deterministic demand increase from broad-based advertising, whereas the rest of the treatments were subject to probabilistic distribution of returns described in Table 3; <sup>3</sup> *Incomplete Information* implies that participants are aware of return distribution of only their own commodity, but not everybody else's; <sup>4</sup> *Government Support* treatment guarantees Government's 1:1 match of the assessment; <sup>5</sup> *Experience with Program* treatment lets subjects experience the returns with Broad-Based advertising.

**Table 2. Questionnaire about the relevant experiment attitudes**

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*Demographic/Academic Questions; Yes/No Questions: (Yes=1, No=0)*

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What is your gender? [Male/Female]

What race are you? [Caucasian/African American/Asian/Hispanic/Other]

What is your college GPA? [GPA<3.0/3.0<GPA<3.5/GPA>3.5]

While I voted yes or no on the referendum, I am actually uncertain. [Yes/No]

*Scale Questions [1-9]: On a scale from 1 to 9, please respond to the following statements to register the extent to which you strongly disagree (1), have equally mixed opinions (5), or strongly agree (9).*

I approve of the fairness of the proposed advertising program.

The proposed advertising program would be profitable for you.

I had enough information about the likely outcomes.

It is the duty of government to pay for advertising that promotes health (anti-smoking, anti-obesity campaigns).

Collective action by all firms in an industry to improve industry profitability is good.

Marketing strategies such as advertising are best done by each individual firm, not the industry.

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**Table 3. Probabilistic Distribution of Broad-Based Advertising Returns for Treatments with Stochastic and Unequal Returns**

<b>Group/Commodity 1</b>	<b>Group/Commodity 2</b>	<b>Group/Commodity 3</b>
20% - $\Delta$ of 0 units	20% - $\Delta$ of 0 units	20% - $\Delta$ of 0 units
20% - $\Delta$ of 0.5 units	20% - $\Delta$ of 1.5 units	20% - $\Delta$ of 2 units
20% - $\Delta$ of 1 units	20% - $\Delta$ of 2 units	20% - $\Delta$ of 3 units
20% - $\Delta$ of 1.5 units	20% - $\Delta$ of 2.5 units	20% - $\Delta$ of 4 units
20% - $\Delta$ of 2 units	20% - $\Delta$ of 4 units	20% - $\Delta$ of 6 units
Expected change in demand = 1	Expected change in demand = 2	Expected change in demand = 3

**Table 4. Descriptive statistics of experiment outcomes by treatment**

Treatment	Control	Equal Returns	Certain Returns	Incomplete Information	Govt. Support	Experience with program
% of "Yes" Votes <sup>1</sup>	0.458	0.604**	0.458	0.542	0.688***	0.622**
Average Part A Profit <sup>2</sup>	3.870	3.681	3.302	4.390	3.407	3.236
Average Part B Profit	3.495	3.818	4.989	4.418	4.207	3.932
Average Part C Profit <sup>3</sup>	N/A	N/A	N/A	N/A	N/A	4.67
Male <sup>4</sup>	0.563*	0.396	0.333	0.208	0.417	0.533
Caucasian	0.438	0.583	0.604	0.646	0.542	0.600
Asian American	0.313	0.292	0.250	0.229	0.313	0.289
African American	0.125	0.083	0.063	0.063	0.063	0.067
Hispanic American	0.042	0.000	0.063	0.021	0.042	0.022
Other	0.083	0.042	0.021	0.021	0.042	0.022
GPA<3.0	0.167	0.104	0.125	0.167	0.063	0.156
GPA>3.5	0.500	0.479	0.500	0.375	0.438	0.622
3.0<GPA<3.5	0.333	0.417	0.375	0.438	0.500*	0.222*
Uncertain About the Program	0.167	0.313	0.292	0.292	0.229	0.267
Fair	5.542	5.083	6.479	5.896	5.563	5.644
Profitable	5.896	5.417	5.938	5.042	5.813	5.600
Enough Information	5.771	5.563	6.104	5.167	6.000	6.378
Government Duty	5.542	6.104	5.917	5.646	6.208	6.489
Collective Action	5.604	6.229	5.896	5.583	5.917	5.867
Individual Firm	6.083	5.604	6.292	5.729	5.688	5.711
Imputed Measures (st. dev. in parentheses):						
Disadvantageous Inequity (DI)	-0.364	(0.886)				
Advantageous Inequity (AI)	1.141	(1.051)				
Risk ( $\sigma$ )	0.114	(0.091)				
# of Subjects	48	48	48	48	48	45

Notes: <sup>1</sup>Asterisks represent whether the means of favourable votes are statistically different relative to control at  $p=0.01$  level (\*\*\*),  $p=0.05$  level (\*\*) or  $p=0.1$  level (\*) of significance. <sup>2</sup>Profits reported in experimental dollars (exchange rate 1 experiment dollar = 2 US dollars). <sup>3</sup>Treatments 1 through 5 did experience profits in part C but they happened after the vote (therefore they are N/A for the vote decision). <sup>4</sup>The rest of the asterisks indicate whether the specific mean is statistically different from overall mean at  $p=0.01$  level (\*\*\*),  $p=0.05$  level (\*\*) or  $p=0.1$  level (\*) of significance. No asterisk indicates that the means are not statistically different. See Table 2 for the units, scales, and explanations of questionnaire items.

**Table 5. Simulated maximum likelihood estimates of fixed and random coefficient models**

	Fixed Coefficient Logit		Random Coefficient Logit	
	Estimate	St. Error	Estimate	St. Error
<b>Inequity and Risk Measures (<math>\alpha, \beta, \theta, S^{DI}, S^{AI}</math>)</b>				
<b>Means:</b>				
Disadvantageous Inequity ( $\alpha_0$ )	-0.066	0.163	4.349***	1.139
Advantageous Inequity ( $\beta_0$ )	0.169	0.206	-4.472***	1.701
Risk ( $\theta_0$ )	-0.460	1.590	-39.101***	11.862
<b>Random Coefficient Functions:</b>				
DI[E( $\pi$ )] ( $\alpha_1$ )			-0.745***	0.192
AI[E( $\pi$ )] ( $\beta_1$ )			0.513**	0.249
Risk[E( $\pi$ )] ( $\theta_1$ )			9.053***	2.323
<b>Standard Deviations:</b>				
Disadvantageous Inequity ( $\varphi_\alpha$ )			3.568***	0.696
Advantageous Inequity ( $\varphi_\beta$ )			12.706***	2.410
Risk ( $\varphi_\theta$ )			31.204***	6.333
<b>Fitted Values:</b>				
$\hat{\alpha}$ : Disadvantageous Inequity	-0.008		0.165	
$\hat{\beta}$ : Advantageous Inequity	-0.021		-1.591	
$\hat{\theta}$ : Risk	0.049		11.754	
<b>Imputed Risk Aversion Measures:</b>				
$S^{DI}$			-0.057	
$S^{AI}$			0.103	
<b>Treatments</b>				
Equity Treatment	0.510	0.427	3.014***	0.884
<b>Treatment Extensions:</b>				
Inequity+Non Stochastic Returns	-0.020	0.435	0.215	0.831
Inequity+Partial Information	0.182	0.503	3.565***	1.183
Inequity+Government Subsidy	0.945**	0.441	5.646***	1.231
Inequity+Experience with Program	0.658	0.454	3.299***	0.858
<b>Other Controls</b>				
Male	0.251	0.263	1.727***	0.588
Ad Influence	0.286	0.338	0.494	0.663
Enough Information	0.096	0.063	-0.224*	0.129
Government Duty	-0.137**	0.573	-0.566***	0.139
Collective Action	0.152**	0.694	0.410***	0.149
Individual Choice	-0.193***	0.615	-0.216*	0.113
Log-Likelihood Function		-180.021		-152.092

Note: \*denotes significance at 10%, \*\*denotes significance at 5%, \*\*\* denotes significance at 1%.

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