INVESTIGATING WAYS TO IMPROVE DISCRETE CHOICE METHODS IN ASSESSING INDIVIDUAL PREFERENCE FUNCTIONS WITH GREATER RELIABILITY AND ACCURACY

BY

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A DISSERTATION SUBMITTED IN PARTIAL FULFILLMENT OF THE REQUIREMENTS FOR THE DEGREE OF

DOCTOR OF PHILOSOPHY

IN

ENVIRONMENTAL AND NATURAL RESOURCE ECONOMICS

UNIVERSITY OF RHODE ISLAND

2007

DOCTOR OF PHILOSOPHY DISSERTATION

OF

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UNIVERSITY OF RHODE ISLAND

2007

ABSTRACT

Assessment of individual preferences is of interest to many disciplines, including economics, marketing, business, and health. Information on individual preferences is the key to understanding and predicting individual and aggregate choice behavior in response to different policy actions and programs, and for evaluating resulting costs and benefits. Whether the objective is to estimate the willingness to pay for changes in attributes of an existing good or for the introduction of a new good with private and public impacts, information on individual preferences is needed. Discrete choice methods have been used for decades for these purposes.

Discrete choice methods involve situations wherein individuals evaluate several policy alternatives and then choose one alternative over others, expressing their willingness to make tradeoffs among alternative attributes. A discrete choice model is then used to recover individuals' preference functions from these responses. In spite of the popularity of discrete choice methods, the reliability and accuracy of these methods in providing correct assessment of individual preferences are often questioned. This dissertation investigates ways to improve the discrete choice method in recovering this information with higher precision. To achieve this, I focus on two essential components of discrete choice experiment implementation: the design process and the econometric analysis process.

Manuscript 1 and Manuscript 2 of this dissertation research focus on the discrete choice experimental design process. Here I specifically address designing stated choice methods, a specific form of discrete choice methods, for valuing public goods.[1] Stated choice methods have become popular among researchers for their ability to value a range of public goods and services and for their ability to

[1]Stated choice methods are also useful for valuing goods and services that are new in the market or when there is insufficient variability in actual choices to allow analysis of the attributes of interest. The focus here is only on public good valuation.

include a range of attributes in the stated choice questions. Stated choice typically involve surveys, which may either take a hypothetical form, wherein respondents are not actually expected to pay for their choices, or a real-money form, wherein respondents make payments for their choices. The incentive properties in both hypothetical surveys and real-money surveys, continue to be a subject of debate. Hypothetical surveys may be subject to hypothetical bias, which could overestimate values of public goods because respondents might not treat monetary costs in hypothetical surveys the same way they treat such costs in their actual daily transactions. On the other hand, real-money surveys may be subject to free-rider bias, which could underestimate the values of public goods because respondents might recognize their opportunity to benefit from the financial contributions of others. These measurement biases may lead to incorrect welfare measures and, therefore, suboptimal policy decisions.

Researchers have developed various theoretical and econometric methods to reduce or correct for these measurement biases. I develop a dominant strategy incentive compatible mechanism for designing stated choice surveys in order to elicit individuals' true preferences, eliminating the incentive to free ride in real-money choice questions. I adapt Clarke's (1) pivotal mechanism to stated choice surveys in order to motivate truth telling. I present theoretical proofs of the incentive compatibility of this mechanism for a binary choice case and a multiple alternative choice case. I design and conduct induced-value experiments to verify if respondents indeed adopt their dominant strategies while faced with the incentive compatible mechanism. I also compare the dominant strategy equilibrium property of my proposed mechanism with that of alternative value revelation mechanisms.

Manuscript 1 discusses the dominant strategy incentive compatible mechanism for a binary choice case, wherein individuals' choice task is to decide whether a public good of fixed size should be provided. Manuscript 2 extends this mechanism to a multiple alternative choice case. For the theoretical demonstration and the experimental application of the multiple alternative choice case, I consider a choice set consisting of four choice alternatives. From this analysis, I find that this proposed mechanism performs quite well for the binary choice case but fails to perform as well in the multiple alternative case. However the study provides a better understanding of individuals' incentives behind incorrect revelation of demand for public goods.

Manuscript 3 deals with the discrete choice modeling process. Unlike Manuscripts 1 and 2, the scope of this analysis ranges beyond stated choice data. Here, I investigate ways to improve discrete choice models that can recover more accurate and reliable welfare measures from the observed responses. Over the years, researchers have continuously developed the modeling procedures in order to improve welfare measures. For over a decade, the research took a direction of accounting for the heterogeneity of the populations, thus providing estimates of willingness to pay (WTP) distributions. One such method, the random parameters logit model have became quite popular in obtaining WTP distributions from the marginal utility distributions for heterogeneous populations. However, most random parameters logit model applications suffer from one limitation because they keep the marginal utility of income constant to obtain the distributions of WTP. I suggest that shifting the distributional assumption from marginal utilities to the welfare measures themselves directly yields the distributions of WTP. An empirical application reveals that this proposed model and the random parameters model yield similar mean WTP, but welfare measure are more readily interpretable in the proposed model.

Finally, Manuscript 4 sums up the findings of this dissertation and discusses the contribution, the limitation and future direction of this research.

ACKNOWLEDGMENTS

I take this opportunity to express my gratitude to all the people and organizations who contributed to the successful completion of my dissertation. First and foremost, I thank my major professor, Dr. Christopher Anderson. I am extremely grateful for his continuous support, inspiration, and his motivation to push the limits and reach for excellence. It was his enthusiasm and interest in novel research ideas that motivated me to take on an interesting and challenging research project, which not only increased my knowledge, but also helped me to learn sophisticated research tools.

I extend my sincere thanks to Dr. Stephen Swallow for his continuous support on both the professional and personal fronts. I appreciate his constant encouragement, experienced guidance, and patience. I thank him for sharing his ideas and channeling my research interests and efforts towards excellence. Finally, I thank him for always being available to listen to my problems and concerns and giving advice. I also extend my thanks to Dr. John Burkett for serving on my committee, reading and providing feedback on my dissertation, sharing ideas about other research projects, and meeting my numerous queries about LaTex. I also express deep gratitude to my other committee members, Dr. James Opaluch, Dr. Cathy Roheim and Dr. Patrick Logan, for their time and assistance. I specifically thank Dr. James Opaluch for his initiative and help in preparing the video conferencing for my defense. I sincerely thank Dr. Paritosh Banerjee, my professor back home, for his help, inspiration and encouragement.

I also thank Drs. James Anderson and Thomas Grigalunas for their guidance and constant encouragement. I am extremely thankful to the Department of Environmental and Natural Resource Economics for funding my study for six years. Thanks are also due to Mrs. Lee Anne McCullough for taking care of all the administrative details.

My sincerest thanks go to my parents, Mr. Madan Gopal Das and Mrs. Bharati Das, for their prayers, blessings, love, and patience all through this time. I also thank my siblings Madhumita and Dipanwita, my brother-in-law Basab, my nephew Rishi (who I parted with at his tender age of one), and my best friend back home, Mrs. Atri Mukherjee, for believing in me and helping me through all the ups and downs of life while away from home. Last, but not least, thanks are due to all the friends that I have made over the years in ENRE and outside ENRE who made my stay in RI a joyful experience. I specifically thank Dr. Samuel Bwalya, Ms. Lydia Napitupulu, Mr. Diego Valderrama, Ms. Enid Kumin, Ms. Gabriela Dobrot, Ms. Jingjie Chu, Dr. Yong Jiang, Mr. Matthew Freeman, Ms. Erica Myers, Dr. Aruna Murty, Dr. Anisha Mendonza, Dr. Prasan Kasturi, and Ms. Shweta Ugaonkar. A special thanks to Dr. Andrada Pacheco for helping me through the frustrating time during the last few months of my study.

DEDICATION

My Parents, Mr. Madan Gopal Das and Mrs. Bharati Das

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Abstract

This paper develops an incentive compatible mechanism for stated choice questions in order to elicit true individual preferences for public goods and services. To achieve this, we adapt Clarke's (1) pivotal mechanism to stated choice questions. We present the theoretical framework of our proposed mechanism and provide a formal proof of its incentive compatibility. We design an induced value experiment to verify the dominant strategy equilibrium property of the same, and compare its performance against the provision point and money back guarantee (PPMBG) with alternative response formats. Although for stated choice questions, our incentive compatible mechanism performs very similarly to the PPMBG mechanism, the theoretical incentive compatibility property provides a motivation for its use in public good valuation.

1.1 Introduction

Stated preference methods or stated choice methods have been used for decades to assess individuals' preferences for a range of public goods and services. Information about preferences is vital in understanding and predicting individual and aggregate behavioral responses to policy actions and to estimate resulting costs and benefits. Stated choice methods involve surveys, wherein individuals evaluate management alternatives and state their preferences to support one alternative over others. These surveys may mimic the choices facing the decision makers, and values may be measured not only in monetary terms but also according to the in-kind tradeoffs that managers face. Econometric analysis proceeds by assuming survey responses reflect tradeoffs that individuals are willing to make without coercion. Using stated choice models (2; 3), econometric results provide a statistical model of how attributes of management actions and costs affect individuals' preferences, and economists use the model to identify monetary measures of values called willingness to pay (WTP) or willingness to accept(WTA). The estimated monetary values then support welfare analysis or provide guidance to policymakers in managing these goods.

Typically, these surveys have been hypothetical, where individuals have been asked to state choices that imply a willingness to pay to support a management action, but the individuals have not actually been expected to pay for that action. While the surveys often are consequential, implying a taxpayers' liability because questions relate to public agency decisions, critics argue that individuals may not treat monetary costs in hypothetical surveys the same way as they treat such costs in their actual daily purchases. Thus individuals may overstate their WTP if they do not fully comprehend the financial obligations behind their decisions. This overvaluation, usually known as hypothetical bias, can leave public managers uncertain about public priorities in public good management.

In order to test for the presence of hypothetical bias in hypothetical surveys, researchers often compare estimated WTP based on respondents' choices in hypothetical questions to estimated WTP based on choices in real-money questions, wherein respondents actually make payments to support a public good. There exist several studies that found presence of hypothetical bias in hypothetical questions (4; 5; 6; 7; 8; 9; 10; 11). For instance, Neill *et al.* (11) in their controlled laboratory experiment found that the WTP in hypothetical questions was significantly higher than the WTP for the same good in a Vickrey auction. Brown *et al.* (7) compared the dichotomous choice and open ended mean WTP under hypothetical and actual payment, and found the mean WTP from hypothetical dichotomous questions to be the highest. Same conclusion was reached by Cummings *et al.* (9) in their comparative study of hypothetical and real discrete choice formats.

However, questions remain concerning the validity of these results because of the presence of free-rider bias in real money questions. While traditional hypothetical surveys may suffer from hypothetical bias, most real-money surveys suffer from free-rider bias, which could underestimate the value of public goods if individuals recognize their opportunity to benefit from the financial contributions of others. For example, Kim and Walker (12), Isaac *et al.* (13) and Poe *et al.* (14) found evidence of free-riding in the context of laboratory experiments. The existence of free-rider bias may cause incorrect interpretation about the presence or extent of this hypothetical and real welfare measurement gap, and consequently may lead to inaccurate policy recommendations. Therefore, for accurate evaluation of this welfare measurement gap, it is necessary to find alternative methods of value revelation that remove or reduce these biases.

Over the years, different theoretical and econometric methods have been developed to reduce or correct for hypothetical and free-rider biases. These include calibration techniques (6; 15; 16; 17), reminding respondents of their budget constraints (11; 18), and using cheap talk design (19; 20; 5). These approaches, though somewhat successful in occasions, failed to settle the debate surrounding hypothetical and free-rider bias. Recent work in this line of research that exhibited promising results has involved questions that establish a minimum threshold of funding in order to provide a public good. In this mechanism, a public good is provided if aggregate contribution is equal to or above the provision point (PP). If it is below the provision point, the good is not provided. Often a money-back guarantee (MBG) is added to the PP as an assurance against the loss of contribution when the public good is not provided. Several studies on PPMBG, spanning over three decades, have found that the provision point and money-back guarantee reduce free-rider bias and significantly improve contribution among respondents (21; 22; 14; 23; 24; 25; 26; 27; 28; 29; 30; 31; 32).[1]

Although previous PPMBG applications found significant reduction in free-ridership among respondents, they cannot claim to eliminate the free-rider bias in real-money questions. Therefore, doubts remain regarding the presence of hypothetical and free-rider bias. This concern led researchers to investigate other institutions that have higher true value revelation potential. Mechanism design literature shows greater promise in this respect.

The task of the mechanism designers is to design mechanisms that eliminate the incentives to misreveal preferences and thus induce individuals to reveal their true preferences. In order to mitigate the free-ridership problem in real-money

[1]Additional assurance can be provided by disbursing the excess contributions over PP to the contributors in some form of rebates (33; 29). These rebates usually take the form of a proportional rebate (PR), where excess contributions are returned to the individuals in proportion to their contributions; extended benefit (EB), where the extra contributions are used to provide more of the public good; or winner-take-all rules, wherein all the excess contributions are returned to one randomly chosen contributor.

stated choice questions, we design a dominant strategy mechanism. Theoretically, it is desirable to design dominant strategy mechanisms, i.e., mechanisms which are non-manipulable. The practical advantage of dominant strategy mechanisms is that an agent does not need to know anything about others' values or strategies in order to choose a best strategy given his own strategies; and therefore implementation is easy. The desirability of such mechanisms is also expressed by Groves and Ledyard ((34), p.56) in their quote, "A fundamental, but generally unstated axiom of non-cooperative behavior is that if an individual has a dominant strategy available, he will use it".

In order to induce true value revelation among individuals, we adapt a special case of the the Vickrey-Clarke-Groves mechanism (35; 1; 36; 37), known as the pivotal mechanism, to stated choice questions.[2] Pivotal mechanism is a dominant strategy mechanism in which true value revelation is always an individual's dominant strategy. In this mechanism, each individual is asked to reveal his valuation of a public good, which may be different from his true valuation of the good. If the aggregate valuation revealed by each individual equals or exceeds the cost of the good then it is provided otherwise it is not provided. An individual is called a pivotal agent if his value revelation reverses the decision based on other members' valuations; not pivotal otherwise. Assuming zero provision cost, a pivotal agent must pay a Clarke tax equal to the absolute value of the aggregate valuation of the good revealed by the other members, and pay nothing if he is not pivotal (38; 39). Pivotal mechanism provides an incentive for the individual to state his value truthfully, because he only has to pay if his value is pivotal, meaning if his stated value makes a pivotal difference in providing or not providing the public

[2]The dominant strategy equilibrium was first discovered by Vickrey (35) and later it was generalized by Groves (36). Clarke (1) and Groves and Loeb (37) independently discovered the dominant strategy mechanism for public goods, commonly known as demand revelation mechanism.

good.

The adaption of Clarke's mechanism to discrete choice framework is not straightforward, because, unlike Clarke's pivotal mechanism, which is based on allowing individuals to state any value, stated choice surveys generally involve discrete choices. Our mechanism design, henceforth is called discrete choice pivotal cost mechanism (DCPCM), and is based only on Clarke's concept of a pivotal agent. It does not take into account Clarke's definition of pivotal tax.

The DCPCM also results in an efficient project choice in the sense that the project is provided whenever the sum of revealed values equals or exceeds the project cost. However, similar to Clarke's pivotal mechanism, this mechanism does not result in fully Pareto-efficient allocations because it fails to satisfy the balanced budget condition (40; 34). Nevertheless, by now it is well-known that it is impossible to design a mechanism for making collective allocation decisions, that are informationally decentralized, non-manipulable, and Pareto optimal. Consequently, researchers settle for second best mechanisms, and neither the Vickrey-Clarke-Groves mechanism nor our proposed mechanism is an exception to this norm.[3]

The paper is organized as follows. In the next section we present the theoretical framework of the mechanism and a formal proof of the incentive compatibility of the DCPCM. Our analysis proceeds by designing an induced-value experiment to verify if a dominant strategy mechanism for discrete choice questions can improve demand revelation in public good valuation and also to test the alternative presentations that yield highest revelation rates. The experimental design is explained in section 3. Section 4 discusses the results, and section 5 concludes the paper.

[3]Maliath and Postlewaite (41), Walker (42), Roberts (43), and Green and Laffont (44) demonstrated the impossibility of designing such an ideal mechanism in the context of resource allocations with public goods.

1.2 The Model

Discrete Choice Pivotal Cost Mechanism

We consider an environment where I agents must collectively decide whether a public project of a fixed size should be provided. The set of feasible project decisions is denoted by $k := \{0, 1\}$, where $k = 1$ implies the project is provided and $k = 0$ implies the project is not provided. An agent i's valuation for a project choice $k \in \{0, 1\}$ is denoted by $v_i(k)$. In a discrete choice framework, an agent is not asked to reveal his valuation $v_i(.)$ for the project; rather he receives a fixed cost $a_i > 0$, which he accepts or rejects depending on his quasi-linear utility function $u_i(k, a_i) = v_i(k) - a_i$. Therefore, the agent has only two possible strategies instead of a continuum of strategies, accepting or rejecting this pre-assigned cost. Let $s_i := \{1, 0\}$ denotes his strategy set, where $s_i = 1$ implies the agent accepts the pre-assigned cost, and consequently his contribution toward the project is taken to be $c_i = a_i$; $s_i = 0$ implying the agent rejects the assigned cost. In this case his contribution toward the project is taken to be $c_i = 0$. Thus, agent i's contribution toward a project can be expressed as:

$$c_i = \begin{cases} a_i & \text{if } s_i = 1 \\ 0 & \text{otherwise.} \end{cases}$$

Given the strategy of agent i, $s_i \in \{1, 0\}$ and the strategies of agents other than i s_{-i}, a social choice function in this environment takes the form $f(s_i, s_{-i}) = (k, t_1, ...t_I : k \in \{1, 0\}), t_i \in \mathbb{R}_+$ for all i, where t_i is the monetary transfer from agent i to the decision making agency. In this environment, the discrete choice pivotal cost mechanism (DCPCM) can be explained by the following two rules.

Rule 1. ***Project Implementation Rule:*** *The public project is provided if the aggregate contribution of all agents equals or exceeds the cost of the project.*

Denoting the cost of the project by $T > 0$, the project implementation rule can be explained as follows:

$$k = \begin{cases} 1 & \text{if } \sum_{i \in I} c_i \geq T \\ 0 & \text{otherwise.} \end{cases}$$

Definition 1. *An agent is pivotal if his contribution changes the project decision based on other members' contributions.*

That is, an agent i is pivotal if:

(1) $\sum_{j \neq i} c_j < T$ and $\sum_{i \in I} c_i \geq T$, where $\sum_{j \neq i} c_j$ denotes the aggregate contribution by all agents other than i.[4,5]

Agent i is not pivotal if:

(2) $\sum_{i \in I} c_i < T$, so the project cannot be provided even with i's contribution or

(3) $\sum_{j \neq i} c_j \geq T$, so the project is provided without i's contribution.

Rule 2. ***Payment Rule:*** *An agent's monetary transfer to the decision making agency t_i is his contribution if he is pivotal; zero if not pivotal.*

$$t_i = \begin{cases} c_i & \text{if } \sum_{j \neq i} c_j < T \text{ and } \sum_{i \in I} c_i \geq T \\ 0 & \text{otherwise.} \end{cases}$$

Rule 1 and Rule 2 that define DCPCM determine the social outcome $f(s_i, s_{-i})$, which in turn determines agent i's payoff as follows:

$$\pi_i(f(.)) = \begin{cases} v_i(k) - t_i & \text{if the project is provided} \\ 0 & \text{if the project is not provided.} \end{cases}$$

Definition 2. *A strategy is a weakly dominant strategy for an agent if it gives him at least as large a payoff as any of his other possible strategy for every possible strategies that his rivals may play.*

[4] An individual gets no benefit when the project is not implemented, that is $v_i(0) = 0$. In discrete choice question format, an agent cannot reveal a negative value; therefore, he is pivotal in only one direction. He can only change the decision from not implementing the project ($k = 0$) to implementing the project ($k = 1$), but not the other way around.

[5] Note, a pivotal agent also implies that the agent has accepted the pre-assigned cost and thus agreed to contribute a positive amount. An agent cannot be pivotal with zero contribution.

That is, if telling the truth s_i^* is a weakly dominant strategy equilibrium, then for all i, $v_i(.)$ and a_i we have:

$$\pi_i(f(s_i^*, s_{-i})) \geq \pi_i(f(\tilde{s}_i, s_{-i})). \tag{1}$$

Proposition 1. *In the discrete choice pivotal cost mechanism, telling the truth s_i^* is a weakly dominant strategy for all i, $v_i(.)$ and a_i.*

Proof. We will prove that when faced with the particular project implementation rule and the payment rule of DCPCM, each agent finds it in his best interest to truthfully answer the discrete choice questions. In this binary discrete choice framework, an agent's truth implies accepting the pre-assigned cost when his net utility is positive ($v_i(1)-a_i > 0$), rejecting it when his net utility is negative ($v_i(1)-a_i < 0$), and remaining indifferent when his net utility is zero ($v_i(1) - a_i = 0$). We have discussed the different pivotal conditions that may arise given the other agents' decisions under definition 1. Here we show that telling the truth is agent i's optimal strategy under each of these social conditions.

Condition 1: Suppose other agents' aggregate contribution implied by s_{-i} is $\sum_{j \neq i} c_j < T$, and agent i's pre-assigned cost a_i is such that $a_i + \sum_{j \neq i} c_j \geq T$. This implies i is pivotal; i.e., i's positive contribution can have a pivotal effect on the project outcome, therefore on his payoff.

In this situation, if $s_i = 1$ then $c_i = a_i$. Since i is pivotal $k = 1$ and $t_i = a_i$. Thus agent i's payoff is, $\pi_i(.) = v_i(1) - a_i$. On the other hand, if $s_i = 0$ then $c_i = 0$. Without i's contribution the project cannot be provided, thus $k = 0$, $t_i = 0$ and his payoff is $\pi_i(.) = 0$. Therefore, given the responses of other agents, the possible outcomes that can arise from alternative strategies adopted by i can be written as,

$$\pi_i(f(s_i, s_{-i})) = \begin{cases} v_i(1) - a_i & \text{if } s_i = 1 \\ 0 & \text{if } s_i = 0. \end{cases}$$

Now, depending on $v_i(.)$ and the pre-assigned cost, a_i, the agent's net utility from the project can be positive, negative, or zero. We will show that when condition 1 is true, telling the truth is the optimal strategy under each possible preference relation.

Case 1: Suppose agent i's net utility is positive, i.e., $v_i(1) - a_i > 0$. Consequently, agent i's payoffs from alternative strategies $\pi_i(f(1, s_{-i})) = v_i(1) - a_i > \pi_i(f(0, s_{-i}))$. Therefore, agent i's best response is to accept the cost; i.e., $s_i^* = 1$, which corresponds to truth revelation.

Case 2: Suppose agent i's net utility is negative; i.e., $v_i(1) - a_i < 0$. Consequently, agent i's payoffs from alternative strategies $\pi_i(f(1, s_{-i})) = v_i(1) - a_i < \pi_i(f(0, s_{-i}))$. Therefore, agent i's best response is to reject the cost, i.e., $s_i^* = 0$, which corresponds to truthful revelation.

Case 3: Suppose agent i's net utility is zero; i.e., $v_i(1) - a_i = 0$. Consequently, agent i's payoffs from alternative strategies $\pi_i(f(1, s_{-i})) = v_i(1) - a_i = \pi_i(f(0, s_{-i}))$. Here agent i is indifferent between accepting or rejecting the cost, thus either $s_i^* = 0$ or $s_i^* = 1$ is a best response and corresponds to truthful revelation.

Therefore, in this pivotal case, we have $\pi_i(f(s_i^*, s_{-i})) \geq \pi_i(f(\tilde{s}_i, s_{-i}))$ for all i and v_i.

Condition 2: Suppose other agents' aggregate contribution $\sum_j c_j$ and agent i's pre-assigned cost a_i are such that $a_i + \sum_{j \neq i} c_j < T$, i is not pivotal.

In this case, the project cannot be provided even if i agreed to contribute toward the project cost. Thus, agent i's decision does not influence the project provision decision or his payoff. Here i's payoff is zero irrespective of his decision. That is, $\pi_i(f(1, s_{-i})) = 0 = \pi_i(f(0, s_{-i})) \; \forall \; i$ and v_i.

Condition 3: Suppose the other agents aggregate contribution $\sum_j c_j$ is such that $\sum_{j \neq i} c_j \geq T$.

In this case, agent i is again not pivotal. The project is provided without i's contribution and i's payoff is v_i irrespective of his decision. Therefore $\pi_i(f(1, s_{-i})) = v_i = \pi_i(f(0, s_{-i})) \; \forall \; i$ and v_i.

Thus, regardless of the pivotal conditions that may arise depending on the responses of other agents s_{-i} , truthful revelation is the weak dominant strategy for agent i, i.e., $\pi_i(f(s_i^*, s_{-i})) \geq \pi_i(f(\tilde{s}_i, s_{-i})) \forall i, v_i, a_i$. This completes the proof. $\square$

1.3 Experimental Design

We demonstrated in the previous section that truthfully answering the discrete choice questions is an individual's weakly dominant strategy while facing the DCPCM. However, before applying the theory in the field to solve actual social allocation problems, it is necessary to test the mechanism in a controlled laboratory environment. Here we explain an induced-value experiment, designed to examine the effectiveness of the DCPCM in motivating individuals to reveal their true preferences.[6]

1.3.1 Treatments

We evaluated the empirical properties of the DCPCM with respect to two alternative demand elicitation formats; one being the provision point money-back guarantee mechanism with continuous response format and the other being the same with discrete choice response format. We denote the former as OE-PPMBG and the latter as DC-PPMBG. We implemented the PPMBG treatment as the control treatment because of its increasing popularity in public good valuation and its success in significantly reducing free-ridership among respondents, both in continuous questions (27; 30; 31) and in discrete choice questions (14; 46; 23). Each session of our experiment started with the OE-PPMBG, and was followed

[6] According to the induced-value theory (45), for the economic experiment to be meaningful the experimenter should assign specific preference functions to each subject and pay them according to the payoffs they receive in the experiments. Our experiment satisfies the induced-value theory.

by a repeat of the OE-PPMBG, the DC-PPMBG, and the DCPCM. We use the OE-PPMBG treatment as the base treatment mainly to familiarize subjects with group decision making in a simple environment. The first OE-PPMBG treatment was for practice purposes only; therefore it was not considered for data analysis. To test the robustness of our mechanism, we changed the orders of the last three treatments between sessions. This ordering helps us to understand if experience has any effect on the dominant strategy plays. This different treatment ordering is explained in table 1.

1.3.2 Parameters

In our experiment, there were 15 subjects in each session assigned to 3 groups of 5 subjects. Before each period, subjects were randomly assigned to a new group, and they were informed of this group assignment process. Subjects participated in one practice period before playing for actual money. To study the subjects' behavioral pattern over time, we repeated our decision making game several times. The first session had 10 periods during each treatment, but the remaining 5 sessions had 15 periods per treatment.

At the beginning of each period, subjects received private values for the public project in question, which were randomly chosen between 5 and 20. In the discrete choice treatments, subjects also received personal levels of cost which ranged from 2.5 (50% of the minimum value) to 30 (150% of the maximum value). This method of cost selection ensures that the distribution of net values resembles the field with a fair number of subjects having positive and negative net values. The project provision cost was kept at 30 experimental dollars throughout the experiment. Each subject knew the range of values, costs, and the provision cost, but they did not know the other subjects' values or costs.[7] Each subject independently decided

[7] Researchers argue that telling subjects about the provision point may lead to equal cost sharing strategy. Rondeau *et al.* (24) studied PPMBG under different information conditions,

whether to contribute toward the cost of the project. Since subjects' positive contributions were allowed to be more than their values, it was possible for them to incur losses in a given period. To take account of this possibility, subjects' show up fees were divided into four parts and given to them before each treatment from which negative payments could be taken off. However, to prohibit subjects from ending the experiment with negative earnings, they were not allowed to contribute more than their endowments. Nevertheless, they were given sufficient initial funds so that fund availability was never a constraint in their contribution decisions.

The experimental design was pre-tested in a pilot session consisting of 10 subjects. In total, 90 students, recruited from the University of Rhode Island, participated in 6 different sessions of the real experiment. Of this student group, 55% were from the undergraduate class "Introduction to Resource Economics" and the remaining were a mix of graduate and undergraduate students with different majors. An experimental session lasted between 1.5 and 2 hours. The total payoff earned by each subject was converted to US dollars according to a pre-determined exchange rate and paid in full at the end of the experiment. The average earning for this experiment was $28.

1.3.3 Instructions

The subjects were provided with written instructions before each treatment, and the instructions were also read aloud. The instructions explained to the subjects the project provision rules, the profit and payment calculation procedures, the cost and value selection criteria, and the group assignment process. They were also given a short quiz immediately following each set of instructions, which was designed to help them understand the rules of the different treatments. The quiz asked the subjects to calculate the social outcomes in some hypothetical example

and they found no evidence of equal cost-sharing among respondents.

scenarios under the alternative payment rules. After the subjects completed the quiz, the experimenter carefully explained the answers to them.

1.3.4 Software

The experiment was programmed with the z-Tree software (47) and was conducted at the simulation laboratory of the University of Rhode Island. The software consisted of a decision screen and a result screen. The decision screen displayed to a subject, his group number, the project cost, his assigned cost (in the discrete choice treatments) and his private benefit from the project if it was implemented. The decision screen also displayed a history table which contained information from all the previous periods including the cumulative profit from each period. A brief summary of the rules of the treatment was also shown to the subjects on the same screen. In the OE-PPMBG treatment, subjects were asked to type their contributions in the appropriate boxes. In the discrete choice treatments, subjects were asked to indicate their decisions to accept or reject the assigned cost by clicking the appropriate button on the screen. All communication among subjects was prohibited during the experiment, except the transmission of responses through computers.

After every subject made their contributions, the results were displayed. The OE-PPMBG result screen displayed to each subject the project cost, his contribution, the group decision, his benefit from the group decision, his payment and profit. Since the decision was discrete in the discrete choice treatments, the results were displayed differently. A subject could see the project cost, his discrete decision ("yes" or "no"), the contribution implied by his decision, the group decision, his benefit from this group decision, payment and profit. In addition, the DCPCM treatment also displayed the decision reached by the other members of his group.

1.4 Results

We discuss our results in the following manner. First, we investigate if the DCPCM treatment was performed according to the theoretical predictions; that is, if individuals did indeed adopt their dominant strategies while facing the DCPCM. Then, we evaluate the performance of the DCPCM with respect to the alternative value revelation methods.

1.4.1 The DCPCM Results

The frequency of dominant strategy plays in the DCPCM treatment is reported in table 2. Overall, respondents behaved according to the theoretical prediction for 83% of the time. As shown in figure 1, the percentage of truth revelation remained consistent across periods, indicating absence of learning effects. The variation in individual behavior within sessions is exhibited in figure 2, where average demand revelation varied from 75.5% to 88.6% among sessions. We also observe that there were 27 (30%) subjects who adopted their dominant strategies in all the periods, and 85 (94.44%) subjects responded truthfully at least half of the time (table 3). Figure 3 illustrates the nature of misrevelation of demand among respondents. The range of net values (induced value-cost), given the distributions of values and costs, is plotted along the horizontal axis of this figure. The vertical axis shows the number of respondents who fell into each range of net values and the number of these respondents who accepted the associated costs for the corresponding range of net values. This figure reveals the existence of both over-revelation and under-revelation of demand. That is, there were some respondents who were free-riding (8.22%) with positive net values and some who were contributing (8.70%) with negative net values against their dominant strategies to do so. Figure 4 explains the proportions of respondents who were adopting different strategies in each period.

This analysis reveals that, though the DCPCM is not perfectly demand revealing, it yields quite impressive results, especially when compared to the previous applications of incentive compatible mechanisms, in particular, the continuous pivotal mechanism. Previous applications of the pivotal mechanism found that the effectiveness of this mechanism depends heavily on how it is presented, with initial approaches finding only 8% to 50% of subjects accurately reporting their values (38; 39; 48; 49). Tideman (50), in his study of the pivotal mechanism with some college fraternities, found that 21% subjects overstating their preferences and 46% understating their preferences. However his finding is not very conclusive because he did not induce the values and also the dominant strategies were explained to the subjects. Attiyeh *et al.* (48) in their controlled laboratory experiment found that in the small group (5 person), only 10% of the time respondents were revealing their true values whereas this percentage was only 8% in the large group (10 person). They concluded that the non-transparent relation between the non-equilibrium behavior and the outcomes might be a reason behind demand misrevelation. In Kawagoe and Mori (39), truth revelation increased from 17% to 47% as more information was provided to the subjects. They offered the weak incentive compatibility of the pivotal mechanism as a possible reason behind misrevelation of demand. Cason *et al.* (38) analyzed the existence of other Nash equilibrium that differ from the dominant strategy equilibrium as a possible cause of the failure of the pivotal mechanism. Our proposed mechanism, despite having all these weaknesses achieved higher demand revelation, implying that these shortcomings are not the actual reasons behind the misrevelation of preferences.

Since the continuous pivotal mechanism failed to perform successfully in the controlled laboratory, it was never implemented in the field to solve real social allocation problem. Therefore, to investigate if the DCPCM has any potential to succeed in the field, we focus on the demand revelation in the first period. The results from the first period of a repetitive experiment can be considered as a close approximation of the expected results from a field application. In experimental applications, respondents rarely achieve equilibrium in the first period, rather their behavior evolves toward equilibrium. However, if subjects take several periods to converge to equilibrium under a given institution, then it is unlikely for that institution to achieve equilibrium in the field. From this respect, our DCPCM yielded promising results, where percentage of demand revelation was 80% in the first period, and it increased by merely 4% at the end of the 15th period.

1.4.2 Model Comparison

Now we focus on the performance of the DCPCM relative to the alternative treatments. As illustrated in table 2 and figure 1, the percentage of truth revelation in the DC-PPMBG treatment is very similar to the same from the DCPCM; whereas the percentage of "yes" responses in the DCPCM is higher than the DC-PPMBG only by an insignificant 3.85%. A closer inspection of the data also reveals that there were 131 responses which can be attributed to free-riding and 88 responses to over-riding in the DC-PPMBG. These figures were 105 and 111 in the DCPCM respectively. This difference in non-dominant strategy plays between treatments is significant at the 3% level (Pearson $\chi^2 = 5.50$).[8] In addition, there were 27 subjects who responded truthfully in all periods in the DCPCM, which is higher than the number of subjects who responded similarly in the DC-PPMBG treatment (table 2). As expected, both discrete choice treatments performed better than the OE-PPMBG treatment. As can be seen in figure 5, and consistent with previous studies, under-revelation of demand was prevalent in the continuous

[8]Note, in the DC-PPMBG free-riding is one of the multiple Nash equilibria (31; 30; 29; 27).

PPMBG treatment, where contribution also fell with repetitions.[9] Moreover, we also find that in discrete choice treatments, subjects were willing to contribute a higher amount more often than in the open-ended questions. The results reveal that a pre-assigned cost above \$10 was accepted in the DCPCM and DC-PPMBG 25% and 23% times respectively, whereas only 7% of the contributions were above \$10 in the OE-PPMBG.[10]

Since each subject in our experiment participated in all four treatments, we are able to study how individuals' responses are influenced by their levels of experience gained through their previous participation history. Table 4 documents the influence of experiences on truth revelation under the discrete choice treatments, and a graphical representation is given in figure 6. The percentage of truth revelations without experience under each treatment are shown in the first 15 periods and without experience in the last 15 periods. In sessions 1 to 3, respondents participated in the DC-PPMBG first and then they participated in the DCPCM, that is, they already had some experience in making decision under a discrete choice environment when they participated in the DCPCM. The truth revelation for sessions 1 to 3 in DC-PPMBG are displayed in the first 15 periods of figure 6, and the same under DCPCM is exhibited from period 16 to 30. The truth revelations in sessions 4 to 6, when DCPCM is done before DC-PPMBG, are displayed in a similar manner in figure 6. This analysis reveals that experience increases truth revelation in both treatments. The demand revelation in the DC-PPMBG (84%) was higher by 4% when it followed the DCPCM (80%), but this change in responses due to experience is not significant (McNemar $\chi^2 = 3.47$). Maximum demand rev-

[9]It is common in voluntary contribution mechanisms for the contributions to decay with repetition (51)

[10]Overall, in the OE-PPMBG treatment, the percentage of aggregate value revealed was 52.64%. On average, 55.18% (figure 1) of the induced values was revealed, with median value revealed at 54.47%. This average is also within the range of 40.2% to 85% obtained in the previous OE-PPMBG studies. Since the objective of this paper is to demonstrate the effectiveness of the DCPCM, we do not exclusively report the results from the OE-PPMBG treatment.

elation occurred when the DCPCM (86%) followed the DC-PPMBG (81%), and this change in responses is significant at the 2% level (McNemar $\chi^2 = 5.53$). From this analysis, we construe that experiences in decision making under similar but simple situations help subjects identify their dominant strategies.

1.4.3 The Logit Model

Next, to investigate which mechanism recovers individual's willingness to pay with higher accuracy, we conduct a logit analysis using the random utility framework. This analysis also enables us to test the hypothesis that an individual's decision to say "yes" increases with higher induced values and decreases with higher costs. In a random utility framework, an individual's utility consists of a known part and a stochastic part. Using such a model, an individual will say "yes" to an assigned cost if his utility from having the project and paying a_i is greater than or equal to his utility from not having the project. In this environment, the probability of a "yes" response by an individual i to an assigned price can be represented as,

$$Pr_i(yes) = Pr\{v_i - a_i + \epsilon_i \geq 0\}, \tag{2}$$

where v_i is individual i's value from having the project and ϵ_i is the random error term. Assuming the error term to be logistically distributed, the respondent's probability of a "yes" response can be written as:

$$Pr_i(yes) = \frac{1}{1 + \exp[-(v_i - a_i)]}. \tag{3}$$

The maximum likelihood estimates of the parameters are given in the first column of table 5 for the DCPCM and in the second column for the DC-PPMBG. As expected, the probability of a "yes" response is positively and significantly related to the induced value and negatively and significantly influenced by the cost.

The mean WTP values, estimated from the DC-PPMBG and the DCPCM, are \$11.98 and \$12.63, respectively, implying similar demand revelation by the discrete choice treatments. Interestingly, these values are very also close to the true mean induced value of \$12.50. Consistent with the previous studies, the mean WTP obtained from the discrete choice questions are almost twice the mean WTP measure of \$6.57 obtained from the OE-PPMBG treatment.[11]

Table 5 also reveals that both models yield very similar coefficient estimates. However, before comparing the parameter estimates obtained from two different treatments, it is necessary to differentiate the influence of the scale difference. Although the scale factor cannot be identified for both data sets, it is possible to identify one scale factor as a ratio of the two. To do this, we estimate a logit model by pooling both data sets and assuming equal parameters but different scale factors. A FIML method is used to efficiently estimate the model parameters and the relative scale factor by simultaneously maximizing a joint likelihood function in GAUSS (53; 54). Interestingly, although the estimated coefficients are similar in values and have the same signs in all the models and thus have similar economic interpretation, a log likelihood ratio test rejects the hypothesis that the DC-PPMBG and the DCPCM have common parameters.[12] On the basis of this statistical analysis, we can interpret that although PPMBG and DCPCM have similar truth revelation rates and model estimates, they do not imply same underlying behavior for the respondents.[13]

[11]In the discrete choice treatments, the mean WTP is estimated by the method suggested by Hanemann (52) that restricts the willingness to pay to be positive:

$$WTP = -(1/\beta_c).ln\{1+\exp(\beta'.x_i)\}. \tag{4}$$

The mean WTP for the OE-PPMBG treatment is estimated by averaging the contributions.

[12]To test the null hypothesis that DC-PPMBG and DCPCM have the same parameter, we calculate the test statistics as $LLR = -2[(L^{PPMBG} + L^{DCPCM}) - L^{Pooled}]$, which follows a chi-square distribution with (|no of common parameters|-1) degrees of freedom (53).

[13]Our intuition is that in a field application where there are more explanatory variables and the variation in their values is also more severe than in the controlled laboratory environment, the DC-PPMBG and the DCPCM might not produce similar model estimates.

1.5 Discussion

The presence of hypothetical and free-rider biases in stated choice surveys leave both researchers and practitioners puzzled about its validity in public good valuation. The purpose of this paper is to address these problems by designing an incentive compatible mechanism for stated choice surveys that eliminates the free-rider bias in real money choice questions. To do this, we adapt Clarke's pivotal mechanism to a binary stated choice case, where individuals decide whether to contribute a fixed amount toward the provision of a fixed-size public good. We present a formal proof of the mechanism and design an induced value experiment to verify the dominant strategy equilibrium of the proposed mechanism.

Our experimental results prove that the DCPCM, though not perfectly demand revealing, performs quite well. Overall, even though DCPCM performs similar to the DC-PPMBG questions in terms of both truth revelation and mean WTP estimates, econometric analysis on the data reveals that these two alternative demand revelation methods do not imply same behavioral responses for individuals. We also find that experience from participating in the DC-PPMBG significantly improves truth revelation in the DCPCM but the converse is not true. Moreover, in comparison to the open-ended questions, the discrete choice treatments are more effective in recovering individuals' preferences.

A closer inspection of our results reveals that among the non-equilibrium strategies, free-ridership incentives are not prevalent. Rather there exists incentives for both over-revelation and under-revelation. This misrevelation of demand in our mechanism cannot be entirely attributed to the ineffectiveness of the mechanism, but it can be explained by a combination of altruism, warm-glow, and confusion. Researches agree that altruism, warm-glow and confusion are prevalent in laboratory experiment and often are reasons behind the failure in attaining the equilibrium. In public good contribution game, people often tend to contribute because they receive utility from the act of giving (warm glow) or when his contribution leads to an increase in the others' payoffs (altruism). Recently, several researchers studied these phenomena in public good games. For example, Palfrey and Prisbrey (55) found evidence of warm glow but rejected the presence of altruism. On the other hand, Anderson *et al.* (56) and Goeree *et al.* (57) found presence of altruism. Andreoni (58) suggested that confusion might also be responsible behind non-dominant strategy plays. Sagoff (59) advocated the idea that individuals might play the role of a citizen while making a social choice who cares more about society than an individual who pursues self interest. Therefore he might contribute out of moral obligation toward the society which might be against his own benefit. Nyborg (60) presented a formal model of multiple preference ordering. Since this area of research is almost unexplored, there is a lack of evidence whether such multiple preference ordering exists.

Now, from the policy point of view, the obvious question that might arise following this analysis is, if both mechanisms perform the same, then which mechanism should be used? The choice between these two mechanisms is not straightforward. On one hand, the incentive compatible property of the DCPCM might provide more credibility to the welfare estimates, and therefore, be preferred by the researchers. On the other hand, analogous to Clarke's pivotal mechanism, the DCPCM suffers from the shortcoming that it might operate with a surplus or a deficit, in which case the funding has to come from taxes elsewhere in the economy. Therefore, we suggest that the mechanism to be used, depends on the specific research objectives. If the research objective is to collect funding for public good provision or management, it is advisable to use DC-PPMBG, because it reduces free riding significantly and also increases contributions. But, the DCPCM should be used if the objective is to inform the decision makers about individuals' true valuations for the public good in question for best management decisions.

Then next obvious question is, how effective will the proposed method be in the field? Previous work on the pivotal mechanism found that more information helps respondents identify their dominant strategies. Attiyeh *et al.* (48) raised the question whether some types of training should be provided to the respondents or whether they should be told about their dominant strategies. While facing the pivotal mechanism, there are two factors that respondents have to understand to identify their dominant strategies; the definition of pivotal condition and the definition of pivotal tax. Intuitively, even if respondents comprehend the concept of pivotal condition, it might be difficult for them to make a mental calculation of the possible pivotal tax that their decisions imply, or it might not be immediately clear to them exactly how much they are pledging toward the public good provision. Kawagoe and Mori (39) also discussed the confusion regarding the calculation of Clarke tax being a possible reason behind the failure of the dominant strategy mechanism. We believe our mechanism would make the understanding of the mechanism easier, since the tax an individual has to pay is either his preassigned cost or nothing. Therefore, we presume that our simple tax definition would facilitate field application and it would help respondents identify their dominant strategies more easily. However, how this mechanism will perform in the field is an empirical question.

Table 1. Treatment orders

Session	Treatment1	Treatment2	Treatment3	Treatment4
1	OE-PPMBG	OE-PPMBG (repeat)	DC-PPMBG	DCPCM
2	OE-PPMBG	DC-PPMBG	OE-PPMBG (repeat)	DCPCM
3	OE-PPMBG	DC-PPMBG	DCPCM	OE-PPMBG (repeat)
4	OE-PPMBG	OE-PPMBG (repeat)	DCPCM	DC-PPMBG
5	OE-PPMBG	DCPCM	OE-PPMBG (repeat)	DC-PPMBG,
6	OE-PPMBG	DCPCM	DC-PPMBG	OE-PPMBG (repeat)

Table 2. Truth revelation in the discrete choice questions

Treatment	Number of true responses	Number of yes responses	Total
DC-PPMBG	1056	628	1275
DCPCM	1059	677	1275

Table 3. Comparing the number of subjects always responding truthfully under the discrete choice treatments

Treatment	Number of subjects always telling the truth	Number of subjects not always telling the truth	Total
DC-PPMBG	22	68	90
DCPCM	27	63	90

Table 4. Order effect on truth revelation

Treatment	Number of true responses	Percentage of true responses
DC-PPMBG (without experience: session 1-3)	488	81%
DCPCM (with experience: session 1-3)	518	86%
DCPCM (without experience: session 4-6)	541	80%
DC-PPMBG (with experience: session 4-6)	568	84%

Table 5. Logit model results

Variable	Coefficient estimates		
	DCPCM	DC-PPMBG	Pooled model
constant	-0.043	-.0200	-0.0410
	(0.218)	(-0.99)	(-0.289)
value	0.483	0.482	0.482
	(15.177)	(15.348)	(21.481)
cost	-0.475	-0.497	-0.484
	(17.239)	(17.449)	(-24.408)
scale			0.980
			(17.230)
Log-likelihood	-605.865	-620.337	-1229.600

The t-statistics are in parentheses.

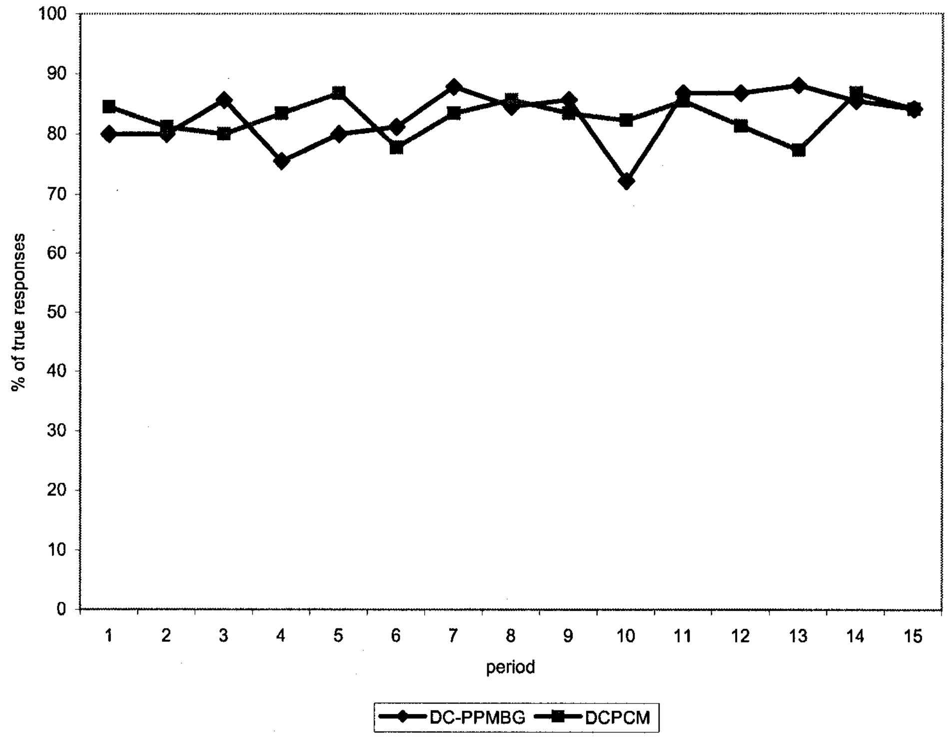

Figure 1. Percentage of truth revelation in the DC-PPMBG and the DCPCM treatments

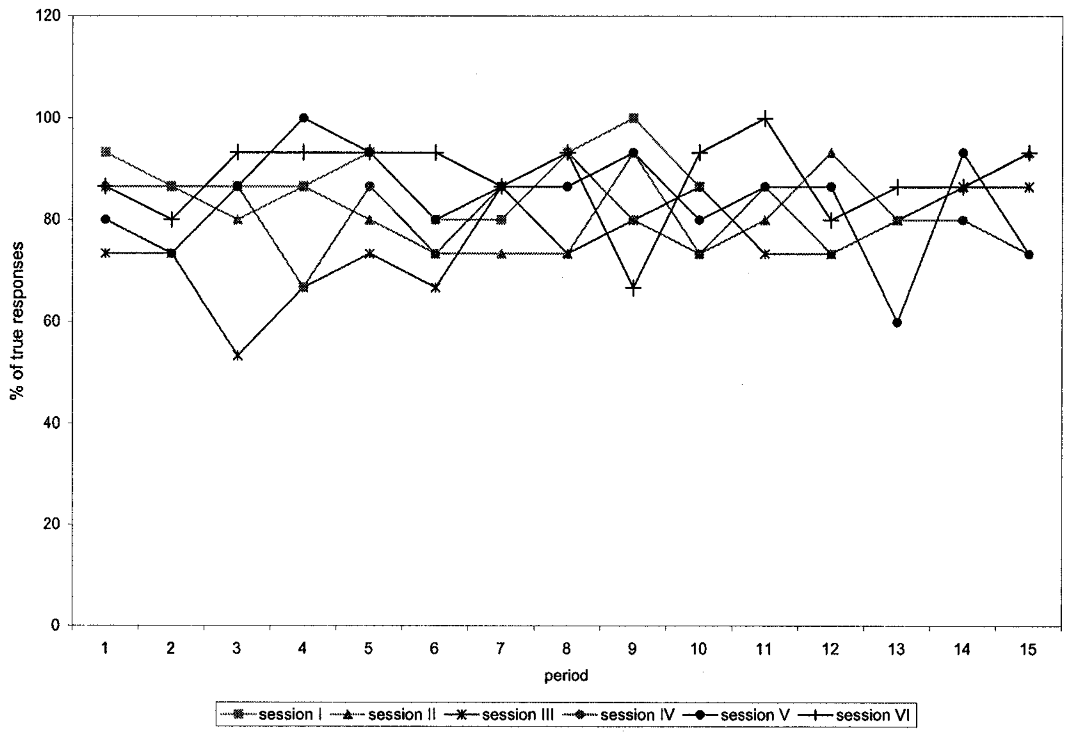

Figure 2. Percentage of truth revelation in the DCPCM by session

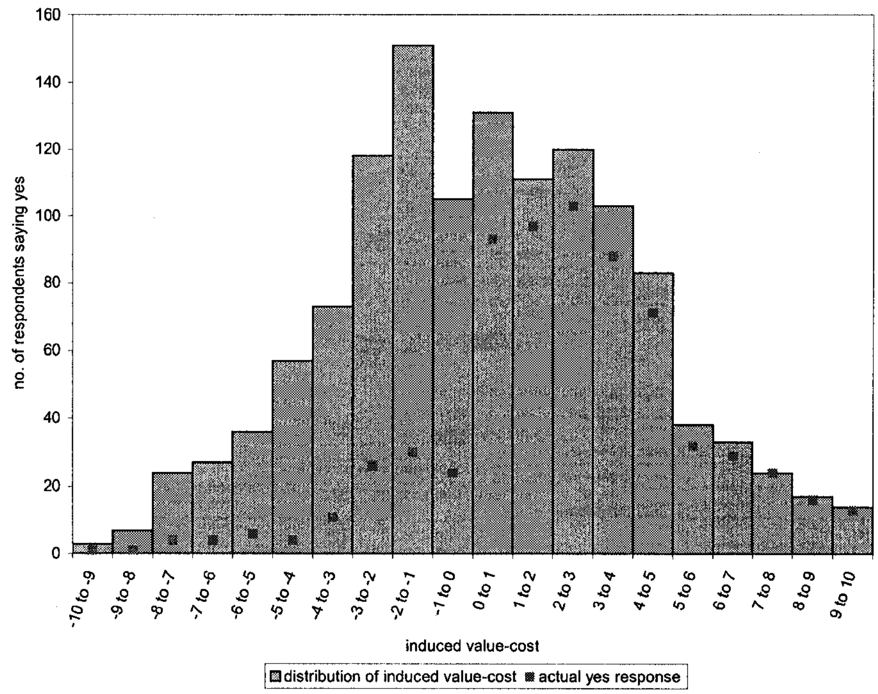

Figure 3. Nature of truth revelation in the DCPCM treatment

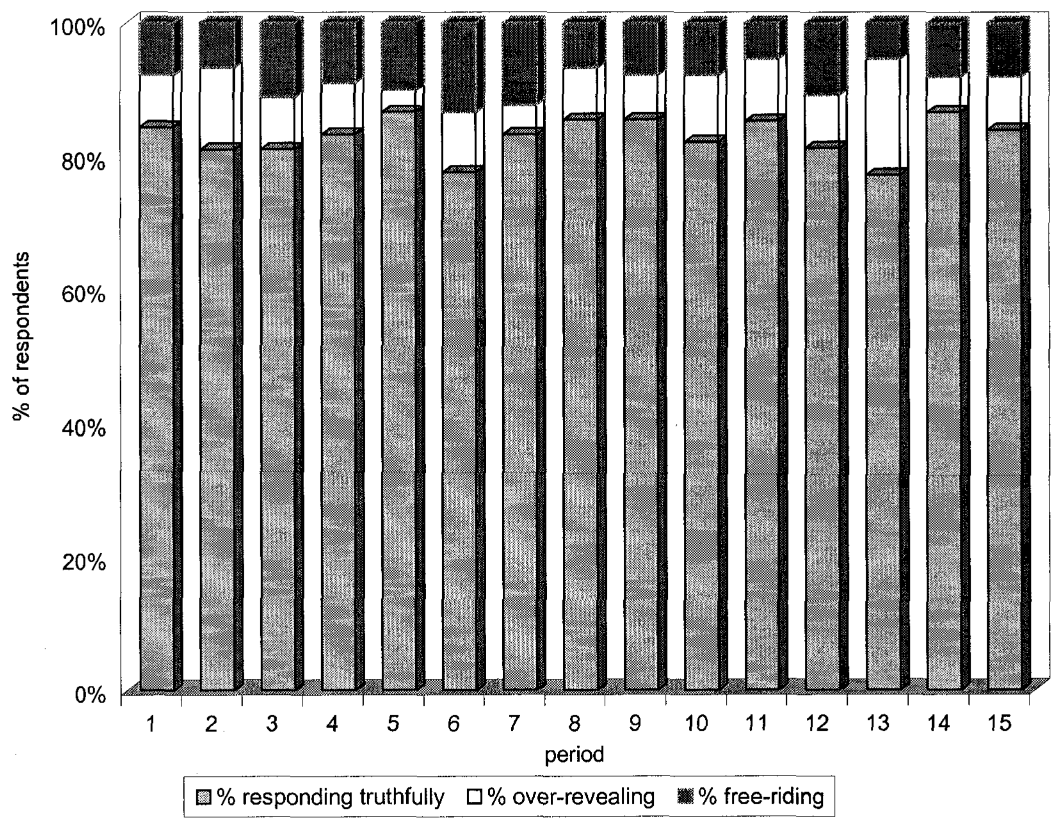

Figure 4. Distribution of different strategy plays in the DCPCM treatment

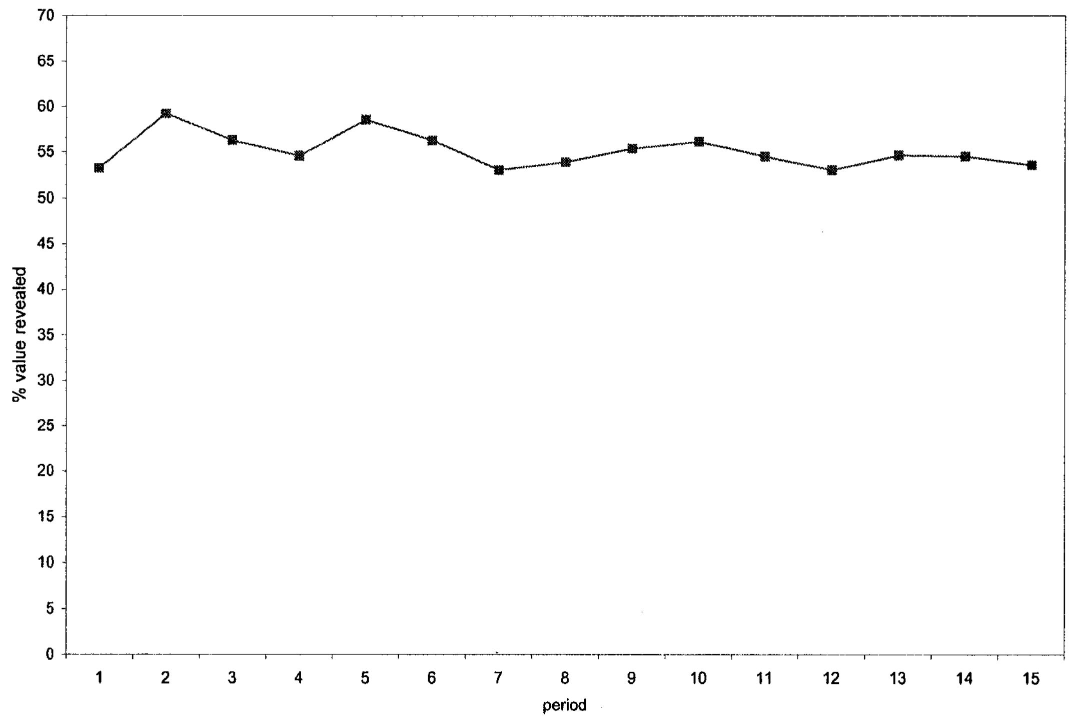

Figure 5. Average percentage of value revealed in the OE-PPMBG

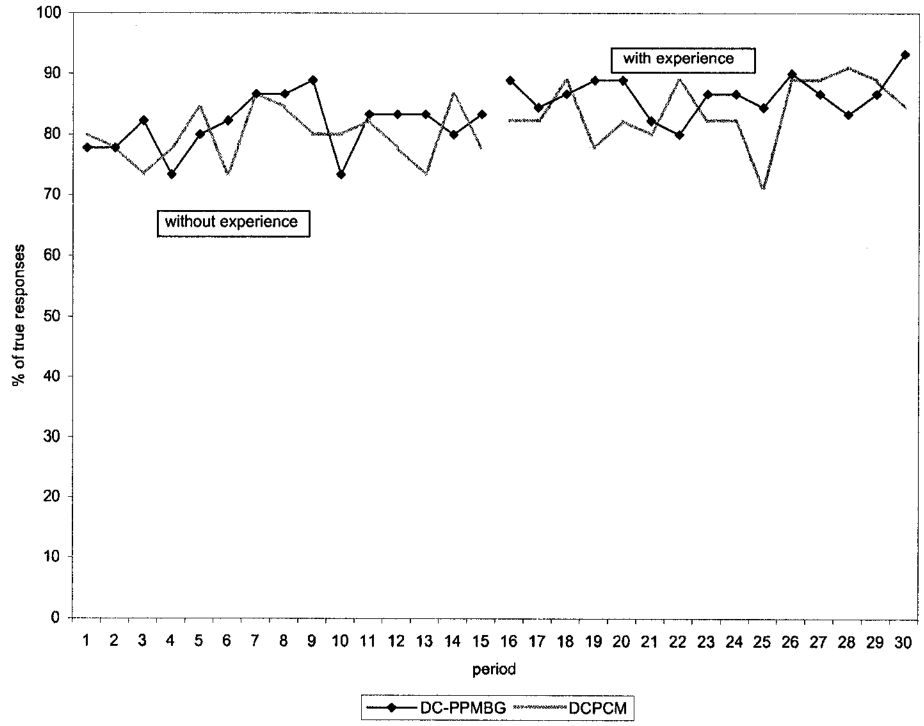

Figure 6. Robustness test of the discrete choice treatments

MANUSCRIPT 2

Incentive Compatible Mechanism Design for Stated Choice Surveys: A Multiple Alternative Choice Case

Abstract

Stated choice surveys, hypothetical or real, have been a valuable tool in eliciting individual preferences for public goods and services for decades. However, the incentive structures of these questions remain to be a potential source of bias in welfare measurements. We design a dominant strategy incentive compatible mechanism that removes the incentives to free ride in real-money choice questions. In previous research (61), we discussed a similar incentive compatible mechanism design for a binary choice case, and in this paper we extend our theory to a multiple alternative choice case. We prove that in this mechanism, truthfully answering the stated choice questions is an individual's dominant strategy. We use experimental tools to verify the model and evaluate its performance with respect to alternative demand revelation mechanisms. For our experimental demonstration, we limit the choice set to four choice alternatives. Although overall our incentive compatible mechanism for the multiple alternative choice case performs quite satisfactorily, it fails to perform as well as the binary choice case.

2.1 Introduction

Stated preference or stated choice methods have been relied upon for eliciting individual preferences for a range of public goods and services. Information on individual preferences is vital in understanding and predicting individual and aggregate choice behavior and in evaluating resulting costs and benefits. Stated choice methods usually involve surveys, wherein individuals evaluate a management alternative and state their preferences to support one alternative over others. Typically, these surveys have been hypothetical wherein subjects are asked to state choices that imply a willingness to pay to support a management action, but the respondents have not actually been expected to pay for that action. Critics argue that respondents might not treat monetary costs in hypothetical questions the same as they treat such costs in actual daily purchases, and this devaluation of costs may lead to over-estimation of public values (4; 5; 6; 7; 8; 9; 10; 11). Several studies have been undertaken to test for the presence of hypothetical bias. For instance, Neill *et al.* (11) in their controlled laboratory experiment found that the willingness to pay in hypothetical questions was significantly higher than the willingness to pay for the same good in a Vickrey auction. Brown *et al.* (7) compared the dichotomous choice and open ended mean WTP under hypothetical and actual payment, and found the mean WTP from hypothetical dichotomous questions to be the highest. Cummings and Taylor (5) found evidence of hypothetical bias for three goods out of four public goods. This presence of "hypothetical bias" can leave public officials uncertain about public values, raising concern about the use of hypothetical surveys in public good management and provision.

In order to validate hypothetical welfare measurements, researchers often compare them against welfare measurements based on real-money choice questions, wherein respondents actually make payments to support a public good. But, these validation methods are open to question because of the presence of free-rider bias in real money surveys. While hypothetical surveys may be subject to hypothetical bias, most real money surveys are subject to free-rider bias, and thus may underestimate willingness to pay measurements because respondents may recognize their opportunity to benefit from others' contributions. For example, Kim and Walker (12), Isaac *et al.* (13) and Poe *et al.* (14) found evidence of free-riding in the context of laboratory experiments. Therefore, for accurate evaluation of this welfare measurement gap or for accurate assessment of individuals' preferences, it is necessary to find alternative methods of value revelation that reduce or eliminate these biases.

Over the years, theoretical and econometric methods that have been developed to reduce or correct for hypothetical and free-rider bias involved calibration of welfare measures (6; 17; 16; 15), reminding respondents of their budget constraints (18; 11) and use of cheap talk design (19; 20; 5). These approaches, though somewhat successful on occasion, failed to settle the debate surrounding the hypothetical and free-rider bias. Recent work in this line of research that exhibited promising results has involved questions that establish a minimum threshold of funding in order to provide a public good. In this mechanism, a public good is provided if aggregate contribution is equal to or above the provision point; if it is below the provision point, the good is not provided. Often a money-back guarantee is added to the provision point as an assurance against the loss of contribution when the public good is not provided. This value revelation method generally is referred to as PPMBG in the literature.

Several studies on PPMBG, spanning over three decades, have found that the provision point and money-back guarantee reduce free-rider bias and significantly improve contribution among respondents (21; 22; 14; 23; 24; 25; 26; 27; 28; 29;

30; 31; 32). Despite their success in reducing free-ridership among respondents, the PPMBG cannot claim to eliminate the free-rider bias in real-money choice questions, and therefore doubts remain regarding the presence of hypothetical and free-rider bias. In this paper, we suggest adopting incentive compatible mechanisms to address the free-ridership problem in real-money choice questions.

Public choice economists consider incentive compatibility as a constraint in making social choice on the basis of individual's self-interest behavior, and consequently attempt to design sophisticated mechanisms to remove individuals' incentives to misreveal their preferences. Although examples of several incentive compatible mechanisms can be found in public choice literature, some of which even perform well in controlled laboratory experiments, making these mechanisms operational is often a daunting task because of their complicated incentive structures. However, among these incentive compatible mechanisms, dominant strategy mechanisms are often preferred from both theoretical and practical standpoints. Theoretically, it is desirable that mechanisms be non-manipulable. This criterion is satisfied by dominant strategy mechanisms. This argument is corroborated by Groves and Ledyard (34),"A fundamental, but generally unstated axiom of non-cooperative behavior is that if an individual has a dominant strategy available, he will use it." Again, since in a dominant strategy mechanism, an agent does not need to know anything about others' valuation or strategies in order to choose a best strategy given his own choices, it is easier to implement.[1] Because of these reasons, we design an incentive compatible dominant strategy mechanism for stated choice surveys in order to induce true demand revelation among respondents. To do this, we adapt a special case of the Vickrey-Clarke-Groves mechanism

[1]Possible violation might occur in a dominant strategy mechanism if agents are able and willing to collude. Such collusive behavior will lead to a failure of the dominant strategy mechanism. In a field application, it is unlikely that such collusive behavior will surface, but likely to emerge in a laboratory application. This collusive nature of agents can be limited by a proper group assignment procedure.

(35; 1; 36; 37), known as the pivotal mechanism, to stated choice surveys.[2] The pivotal mechanism is incentive compatible in dominant strategies, where reporting one's preference truthfully is always a dominant strategy.

In a previous paper, we discussed a similar dominant strategy incentive compatible method for a binary choice case (61). We presented a theoretical proof of the mechanism in an environment where an agent is allowed to contribute a fixed amount or nothing toward the provision of a public good of fixed size. In this mechanism, alternatively called DCPCM, an agent pays the fixed amount if he is pivotal, otherwise he pays nothing. We designed induced-value experiments to validate our theory and compared its performance with the provision point money-back guarantee mechanism. The demand revelation of our mechanism was 83%, which was higher than the previous continuous pivotal mechanism applications. This promising result from the binary choice analysis gave us hope that the mechanism has potential to deal with more complex design issues we ultimately wish to address.

Here, to recover true preferences of individuals for a multiple alternative choice case, we extend the DCPCM to a multiple alternative choice case. Although there are a few experimental and field studies on pivotal mechanisms, they are mostly focused on a binary choice case (38; 39; 48; 50) or a continuous choice case (62). Tideman and Tullock (63) were first to discuss the pivotal mechanism with more than two alternatives. Later, Tideman (50) studied the pivotal mechanism involving both multiple options and pairs of options in a real economic system, where college fraternities participated in a collective decision making process in their weekly meetings. He found that subjects were more interested in revealing

[2]Vickrey's (35) paper on dominant strategy equilibrium for private goods with quasi-linear utilities motivated the later research on incentives. Groves (36) later developed a generalization of this mechanism. Vickrey's dominant strategy mechanism for public goods was independently developed by Clarke (1) and Groves and Loeb (37), generally known as a demand revelation mechanism.

their demand for their most preferred alternative.

The adaption of Clarke's mechanism to stated choice framework is not straightforward, because, unlike Clarke's pivotal mechanism, which is based on allowing individuals to state any value, stated choice surveys generally involve discrete choices. Our mechanism design is only based on Clarke's concept of a pivotal agent, and does not take into account Clarke's definition of pivotal tax. Again, while designing DCPCM for a multiple alternative choice case, there are several issues that needed to be addressed. In a multiple alternative choice case, if individuals are asked to choose one alternative out of several alternatives, then nothing is known about their preferences for the other alternatives in the choice set. For the dominant strategy property of the DCPCM to hold with multiple alternatives, we need to know individuals' preferences for all alternatives. Therefore, we allow individuals to choose all the alternatives they prefer. In other words, the choice set consists of all the available project alternatives and all their possible combinations. Moreover, when there is a possibility of providing more than one public good, then there might exist a substitute or complementary effect among alternatives, which might also eliminate the dominant strategy property. To deal with this problem, we impose restrictions on the utility functions of individuals, which we discuss in detail in the model section.

Here we should also point out that, the DCPCM achieves efficient project choice, in the sense that the project is provided whenever the sum of revealed values equals or exceeds the project cost. However, analogous to Clarke's pivotal mechanism, our mechanism operates with either a surplus or a deficit, so Pareto efficiency is not achieved.[3] Regarding this efficiency cost, Vickrey (35) commented that modifying the mechanism to reduce or eliminate this cost is not possible

[3]Several researchers, in the context of public good allocation, proved that it is impossible to design mechanisms for making collective allocation decisions that are informationally decentralized, non-manipulable, and Pareto optimal (41; 42; 43; 44).

without using some prior information or violating the demand revelation property. Consistent with this line of research, our dominant strategy mechanism allows inefficiency. In the next section we discuss our model. Section 3 discusses the experimental design, section 4 presents the results, and section 5 concludes the paper.

2.2 The Model

Discrete Choice Pivotal Cost Mechanism

We consider an environment where I agents must collectively decide which project or projects from a set of finite public projects k should be provided. For the exposition here, we assume that there are two possible project alternatives, and agents must decide whether to provide one of the projects, both projects, or none of the projects. Denoting the two possible projects as A and B, the set of feasible projects can be denoted by $k := \{A, B, A+B, 0\}$, where $k = 0$ implies no project is provided and $k = A + B$ implies both projects are provided. An agent i's value for a project choice k is $v_i(k)$. An agent gets no benefit when the project is not provided, that is $v(0) = 0$. In a discrete choice framework, we do not ask an agent to reveal his valuation $v_i(.)$, rather, the agent is informed of a proposed cost for each project, $a_i^k > 0$. Then he makes a project choice from the set k depending on his quasi-linear utility function $u_i(k, a_i) = v_i(k) - a_i^k$. [4] To eliminate substitution and complementary relation possibilities among project alternatives, we assume that utilities are strongly separable on project alternatives, which, in turn, implies $u_i(A+B, a_i^A, a_i^B) = u_i(A, a_i^A) + u_i(B, a_i^B)$.

In this environment, an agent has four possible strategies, A, B, both projects, or none of the projects. Let i's strategy set be $s_i := \{A, B, A+B, 0\}$, where $s_i = A$ implies the agent chooses project A and his contribution toward project A is taken

[4] When $k = A + B$, $a_i^{A+B} = a_i^A + a_i^B$ and when $k = 0$, $a_i^0 = 0$.

to be a_i^A and toward project B as a_i^B or more formally,

$$c_i^A = \begin{cases} a_i^A & \text{if } s_i = A \\ 0 & \text{otherwise.} \end{cases}$$

similarly, his contribution toward project B:

$$c_i^B = \begin{cases} a_i^B & \text{if } s_i = B \\ 0 & \text{otherwise.} \end{cases}$$

When agent i chooses both projects $s_i = A + B$, then his total contribution is taken to be $c_i^A + c_i^B = a_i^A + a_i^B$.

Given the strategy of agent i, s_i, and the strategies of agents other than i, s_{-i}, a social choice function in this environment takes the form $f(s_i, s_{-i}) = ((k, t_1, ...t_I) : k \in \{A, B, A+B, 0\}), t_i \in \mathbb{R}_+$ for all i, where t_i is the monetary transfer from agent i to the decision-making agency. In this environment, the discrete choice pivotal cost mechanism (DCPCM) can be explained by the two rules explained below.

Rule 3. ***Project Implementation Rule:*** *A public project is implemented if the aggregate contribution from all agents for a project equals or exceeds the cost of that project.*

Denoting the cost of project A by T^A and project B by T^B, the project outcome is determined as follows:

$$k = \begin{cases} A & \text{if } \sum_{i\in I} c_i^A \geq T^A \text{ and } \sum_{i\in I} c_i^B < T^B \\ B & \text{if } \sum_{i\in I} c_i^A < T^A \text{and } \sum_{i\in I} c_i^B \geq T^B \\ A+B & \text{if } \sum_{i\in I} c_i^A \geq T^A \text{ and } \sum_{i\in I} c_i^B \geq T^B \\ 0 & \text{otherwise.} \end{cases}$$

Definition 3. *An agent is pivotal if his contribution changes the project decision based on other members' contributions.*

In this multiple alternative choice situation, an agent i can be pivotal in the provision of project A, B, or both.[5] Denoting the aggregate contributions of agents other than i as, $\sum_{j \neq i} c_j^A$ for project A and $\sum_{j \neq i} c_j^B$ for project B, we can write the different pivotal conditions as follows:

(PC1) Suppose $\sum_{j \neq i} c_j^A < T^A$, $c_i^A + \sum_{j \neq i} c_j^A \geq T^A$ and $\sum_{j \neq i} c_j^B \geq T^B$. This implies i's contribution can have a pivotal effect on the implementation of project A; therefore, agent i is pivotal in A. The contributions for project B from other agents are such that it can be provided without i's contribution. Therefore, i is not pivotal in B.

(PC2) $\sum_{j \neq i} c_j^A < T^A$, $c_i^A + \sum_{j \neq i} c_j^A \geq T^A$, and $\sum_{i \in I} c_i^B < T^B$. This implies project A cannot be provided without i's contribution and project B cannot be provided even with i's contribution. Therefore, agent i is pivotal in A and once more not pivotal in B.

Similarly, PC3 and PC4 present situations when agent i is pivotal in B but not in A.

(PC3) $\sum_{j \neq i} c_j^B < T^B$, $c_i^B + \sum_{j \neq i} c_j^B \geq T^B$, and $\sum_{j \neq i} c_j^A \geq T^A$.

(PC4) $\sum_{j \neq i} c_j^B < T^B$, $c_i^B + \sum_{j \neq i} c_j^B \geq T^B$, and $\sum_{i \in I} c_i^A < T^A$.

(PC5) Now consider the situation when i's preassigned costs are such that positive contributions from him have a pivotal effect in the implementation of both projects, which, in turn, implies: $\sum_{j \neq i} c_j^A < T^A$, $c_i^A + \sum_{j \neq i} c_j^A \geq T^A$ and $\sum_{j \neq i} c_j^B < T^B$, $c_i^B + \sum_{j \neq i} c_j^B \geq T^B$. In this case, neither of the projects can be provided without i's contribution. Therefore, i is pivotal in both.

Rule 4. ***Payment Rule:*** *According to the discrete choice pivotal cost mechanism*

[5] In discrete choice question formats, an individual cannot reveal a negative value; therefore, he is pivotal in only one direction; i.e., he can only change the decision from not implementing a project to implementing it, but not the other way around.

(DCPCM), an agent's monetary transfer to the central agency is his contribution if he is pivotal and zero if not pivotal.

Agent i's monetary transfer is determined as follows:

$$t_i = \begin{cases} c_i^A & \text{if } i \text{ is pivotal in project } A \text{ but not in project } B \\ c_i^B & \text{if } i \text{ is pivotal in project } B \text{ but not in project } A \\ c_i^A + c_i^B & \text{if } i \text{ is pivotal in both projects} \\ 0 & \text{otherwise.} \end{cases}$$

Rule 1 and Rule 2 that define DCPCM, determine the social outcome $f(.)$, which determine agent i's payoff as follows:

$$\pi_i(f(.)) = \begin{cases} v_i(k) - t_i & \text{when } k \neq 0 \\ 0 & k = 0. \end{cases}$$

Definition 4. *A strategy is a weakly dominant strategy for an agent if it gives him at least as large a payoff as any of his other possible strategies for every possible combination of strategies that his rivals may play.*

That is, if telling the truth s_i^* is a weakly dominant strategy equilibrium for all i, $v_i(.)$ and a_i, then we have:

$$\pi_i(f(s_i^*, s_{-i})) \geq \pi_i(f(\tilde{s}_i, s_{-i})). \tag{5}$$

Proposition 2. *In the discrete choice pivotal cost mechanism, telling the truth s_i^* is a weakly dominant strategy equilibrium for all i, $v_i(.)$ and a_i^k.*

Proof. The intuitive idea behind the notion that the DCPCM truthfully implements social choice function $f(.)$ in dominant strategies is that in this mechanism, each agent i finds telling the truth s_i^* better than playing any other strategy for any choices by other agents. In this multiple project alternative case, an agent's preference functions from the alternatives might be such that he prefers one of the alternatives (1) $u_i(A, a_i^A) > 0$ and $u_i(B, a_i^B) < 0$ or (2) $u_i(B, a_i^B) > 0$ and $u_i(A, a_i^A) < 0$; both alternatives (3) $u_i(A, a_i^A) > 0$ and $u_i(B, a_i^B) > 0$; or none of the alternatives (4) $u_i(A, a_i^A) < 0$ and $u_i(B, a_i^B) < 0$. An agent's truthfulness implies choosing A when (1) is true, choosing B when (2) is true, choosing both when (3) is true, and choosing none when (4) is true.[6] We consider all these cases to show that truth telling is indeed an optimal strategy for a respondent given the strategies of other respondents. This is equivalent to showing that for any preference relations and pivotal conditions, equation (5) is true.

We discussed all pivotal situations that can arise from all combinations of strategies adopted by i and other agents but i under definition 3. Now, given other agents' decisions s_{-i}, if agent i's pre-assigned costs are such that he is pivotal in at least one project, then i's decision has a pivotal effect on the provision decision of that project, and thus on his payoff. We have discussed that agent i has four possible strategies to choose from, with one of them being weakly dominant. In the following table, we list agent i's payoffs that arise under alternative strategies adopted by i and under possible pivotal conditions. Since i can be pivotal in five ways, there are five different social outcomes depending on i's strategies. We indicate the strategies adopted by agents other than i by s_{-i}^p when their strategies are such that i is pivotal in at least one project, and by s_{-i}^{np} when i is not pivotal in any of the projects.

Social Outcomes	$\pi_i(f(A, s_{-i}^p))$	$\pi_i(f(B, s_{-i}^p))$	$\pi_i(f(A + B, s_{-i}^p))$	$\pi_i(f(0, s_{-i}^p))$
SC1(PC1)	$v_i(A) - a_i^A + v_i(B)$	$v_i(B)$	$v_i(A) - a_i^A + v_i(B)$	$v_i(B)$
SC2(PC2)	$v_i(A) - a_i^A$	0	$v_i(A) - a_i^A$	0
SC3(PC3)	$v_i(A)$	$v_i(A) + v_i(B_i) - a_i^B$	$v_i(A) + v_i(B) - a_i^B$	$v_i(A)$
SC4(PC4)	0	$v_i(B) - a_i^B$	$v_i(B) - a_i^B$	0
SC5(PC5)	$v_i(A) - a_i^A$	$v_i(B) - a_i^B$	$v_i(A) - a_i^A + v_i(B) - a_i^B$	0

Consider SC1 which arises when PC1 is true; that is, when i is pivotal in

[6] If net utility from a project is zero, then an agent is indifferent between choosing or not choosing that project, and either response is considered to be a true response.

A but not in B. Suppose agent i chooses project A or $s_i = A$; this also implies $c_i^A = a_i^A$ and $c_i^B = 0$. Since i is not pivotal in B and project B is provided without i's contribution, we have $k^* = A + B$ and $t_i = a_i^A$ and accordingly $\pi_i(f(A, s_{-i}^p)) = v_i(A) - a_i^A + v_i(B)$. Now suppose i chooses project B then $s_i = B$, and $c_i^B = a_i^B$ and $c_i^A = 0$. Since project A cannot be provided without i's contribution and B can be provided without i's contribution, we have $K^* = B$ and $t_i = 0$, and consequently $\pi_i(f(B, s_{-i}^p)) = v_i(B)$. Now if i chooses $A + B$, then $c_i^A = a_i^A$ and $c_i^B = a_i^B$, $k^* = A + B$ and $t_i = a_i^A$ and his payoff, $\pi_i(f(A + B, s_{-i}^p)) = v_i(A) - a_i^A + v_i(B)$. If $s_i = 0$, then $K^* = B$ and consequently $\pi_i(f(0, s_{-i}^p)) = v_i(B)$. These payoffs are shown in row 1 of the table. Similarly i's payoff can be calculated under PC3 when i is pivotal in B but not in A.

Now consider pivotal condition PC2. Suppose agent i chooses project A and thus $s_i = A$. This implies $c_i^A = a_i^A$ and $c_i^B = 0$ and $k^* = A$, $t_i = a_i^A$, and accordingly $\pi_i(f(A, s_{-i}^p)) = v_i(A) - a_i^A$. If i chooses B, then $c_i^B = a_i^B$ and $c_i^A = 0$. Since a_i^B is such that project B cannot be provided even with i's contribution, we have $k^* = 0$ and $t_i = 0$. Therefore, $\pi_i(f(B, s_{-i}^p)) = 0$. If i chooses $A + B$, then $k^* = A$ and $t_i = a_i^A$ and his payoff, $\pi_i(f(A + B, s_{-i}^p)) = v_i(A) - a_i^A$. If $s_i = 0$ then $K^* = 0$ and consequently $\pi_i(f(0, s_{-i}^p)) = 0$. These payoffs are shown in row 2 of the table. Payoffs in row 4 are calculated in similar fashion under PC4 when i is pivotal in B but not in A.

Now, consider the case when i is pivotal in both projects, as indicated by the pivotal condition PC5. In this case, suppose agent i chooses project A, $s_i = A$. This implies $c_i^A = a_i^A$ and $c_i^B = 0$. Therefore, $k^* = A$ and $t_i = a_i^A$ and accordingly $\pi_i(f(A, s_{-i}^p)) = v_i(A) - a_i^A$. If i chooses B then $k^* = B$, $t_i = c_i^B$, and $\pi_i(f(B, s_{-i}^p)) = v_i(B) - a_i^B$. If i chooses $A + B$, then $k^* = A + B$, $t_i = a_i^B + a_i^B$, and his payoff $\pi_i(f(A + B, s_{-i}^p)) = v_i(A) - a_i^A + v_i(B) - a_i^B$. If $s_i = 0$, then $K^* = 0$

and consequently $\pi_i(f(0, s^p_{-i})) = 0$. These payoffs are shown in row 5 of the table.

We show that agent i's best response is to tell the truth under all the above mentioned situations for any preference relations.

Case 1: Consider the case where agent i's v_i and the pre-assigned costs a_i are such that $v_i(A) - a_i^A > 0 > v_i(B) - a_i^B$, which also implies that $v_i(A) - a_i^A > v_i(A) - a_i^A + v_i(B) - a_i^B$. Agent i's best response is $s_i^* = A$, since project A gives him the highest net benefit. Comparing the payoffs under all possible strategies and social outcomes from the table, we can see that $\pi_i(f(A, s^p_{-i})) \geq \pi_i(f(\tilde{s}_i, s^p_{-i}))$. Therefore, agent i's best response is to choose project A, which corresponds to truth revelation.

Case 2: Similarly, we can show that when $v_i(B) - a_i^B > 0 > v_i(A) - a_i^A)$, $\pi_i(f(B, s^p_{-i})) \geq \pi_i(f(\tilde{s}_i, s^p_{-i}))$ is true. Once again, agent i's best response is to tell the truth.[7]

Case 3: Now consider the case when individual's v_i and the pre-assigned costs, a_i are such that, $v_i(A) - a_i^A > 0$ and $v_i(B) - a_i^B > 0$. This also implies, $v_i(A) - a_i^A + v_i(B) - a_i^B > v_i(A) - a_i^A$ and $v_i(B) - a_i^B$. Again comparing payoffs under all conditions and all strategies from the table, it is clear that $\pi_i(f(A + B, s^p_{-i})) \geq \pi_i(f(\tilde{s}_i, s^p_{-i}))$. Here the best response is, $s_i^* = A + B$.

Case 4: Consider the case where an individual's v_i and the pre-assigned costs, a_is are such that, $v_i(A) - a_i^A < 0$ and $v_i(B) - a_i^B) < 0$. Here the best response is, $s_i^* = 0$. Comparing payoffs under all conditions and all strategies, we find that $\pi_i(f(0, s^p_{-i})) \geq \pi_i(f(\tilde{s}_i, s^p_{-i}))$.

When $\sum_{j \neq i} c_j^A$, $\sum_{j \neq i} c_j^B$ and a_i are such that i is not pivotal, then i's decision does not have any influence on the project provision decision and therefore on his payoff. In this non-pivotal case, we have, $\pi_i(f(s_i^*, s^{np}_{-i})) = \pi_i(f(\tilde{s}_i, s^{np}_{-i}))$ for all s_i, a_i and v_i.

[7] In this case, $v_i(B) - a_i^B > v_i(A) - a_i^A + v_i(B) - a_i^B$.

Thus, regardless of the strategies of other agents s^p_{-i} and s^{np}_{-i} , truthful revelation is a best response for agent i; i.e., $\pi_i(f(s_i^*, s_{-i})) \geq \pi_i(f(\tilde{s}_i, s_{-i}))$. This completes the proof. $\square$

2.3 Experimental Design

It is important to note that just because a mechanism has nice theoretical properties does not necessarily mean that it will be effective in solving a real social allocation dilemma. To test its tractability in practice, it is essential to validate the mechanism with appropriate laboratory experiments. If a mechanism fails to perform in a simple, controlled laboratory environment, then it is unlikely to succeed in the field. Here we design an induced-value experiment to verify if individuals do indeed adopt their dominant strategies while facing the DCPCM. For our experiment, we consider two potential public projects denoted as project A and project B. The task of a subject is to decide whether to contribute toward project A, project B, both projects, or none of the projects.

2.3.1 Treatments

We evaluate the empirical properties of the DCPCM relative to the provision point and money-back guarantee mechanism (PPMBG). We used three different formats of the PPMBG: a continuous response PPMBG with one alternative, a continuous response PPMBG (OE-PPMBG) with multiple alternatives, and a discrete choice response PPMBG (DC-PPMBG) with multiple alternatives. Each session started with the binary OE-PPMBG followed by the OE-PPMBG, the DC-PPMBG, and the DCPCM treatment with four choice alternatives. The ordering of the last three multiple alternative choice treatments varied among sessions. This treatment ordering is explained in table 6. We used the binary OE-PPMBG as the base treatment in order to familiarize subjects with voluntary types of games in a simple decision-making environment. The results from this treatment were not used for data analysis.

2.3.2 Parameters

In each session of our experiment, 15 subjects in groups of 5 participated in collective decision-making games under alternative market institutions. In order to prohibit any sort of virtual cooperation among the respondents, the groups were randomly shuffled between periods. To study the evolution of subjects' behavioral responses overtime, we repeated each treatment fifteen times. Subjects participated in one practice period before playing for real money.

At the beginning of each period, subjects were informed of their values and costs (in the discrete choice treatments) for each project. Values were randomly chosen between 5 and 20, and the costs ranged from 2.5 (50% of the minimum value) to 30 (150% of the maximum value). The costs were selected in such a fashion that the laboratory environment closely approximates the field condition where subjects have both positive and negative net values for public projects. The costs and values were drawn from the same distribution for each project. Subjects knew the range of costs and values, but they did not know the other subjects' valuations or costs. Throughout the experiment, the project provision cost was fixed at 30 experimental dollars for each project, and subjects were informed of the project costs.[8] Subjects' positive contributions were allowed to be more than their values, but not more than their total profits. This admits the possibility of negative earning at the end of a period, but prohibits negative profit at the end of a treatment. Subjects' show-up fees were divided into four parts and given to each subject before each treatment from which negative earnings could be taken

[8]The project provision costs were kept the same to avoid any confusion that may arise if subjects receive a high assigned cost for a low-cost project and a low assigned cost for a high-cost project.

off. Subjects were given enough initial funding so that the availability of funds was never a constraint in their contribution decisions.

Before running the real experiment, we tested the experimental design on a pilot session consisting of 10 subjects. 60 students recruited from the graduate and undergraduate classes of the University of Rhode Island participated in 4 different sessions of the real experiment. An experimental session took between 1.5 hours to 2 hours to conclude. The total profit earned by each subject was converted to US dollars according to a predetermined exchange rate and paid in full at the end of the experiment. The average payment for this experiment was $24.

2.3.3 Instructions

The subjects were provided with written instructions before each treatment, and the instructions were also read aloud. The instructions explained the project provision rules, group assignment process, value and cost selection criteria, and the profit and payment calculation criteria for each treatment. Subjects were also given a short quiz immediately following each set of instructions, which was designed to help them understand the rules of the different treatments. The quiz asked subjects to calculate the social outcomes in some example scenarios under the alternative payment mechanisms. After subjects completed the quiz, the experimenter carefully explained the answers to them.

2.3.4 Software

The software was accessed via a decision screen and a result screen. The decision screen displayed the subject's group number, the project cost, his assigned cost (in the discrete choice treatments), and the benefit he would receive if the project was implemented. This information was displayed separately for each project. The decision screen also displayed a history table which contained information from all the previous periods and the cumulative profit from each period. A brief summary about the rules of the treatment was also shown to the subjects on the same screen. In the OE-PPMBG treatment, subjects were asked to write their contributions in the appropriate boxes. In the discrete choice treatments, subjects were asked to indicate their decisions to accept or reject the assigned costs for the projects by clicking the appropriate button on the screen. After subjects made their decisions, they could immediately see the total amount they were contributing toward each project. This allowed them to see the amount they would have to pay if one or both projects were implemented. If both projects were implemented and the subjects accepted the assigned costs for both projects, then their payment was the sum of payments for each project and their benefit was the sum of the benefits received from each project.

After all subjects made their contribution decisions, the results were displayed. The OE-PPMBG result screen displayed to a subject the project cost, his contribution, the group decision, his benefit from the group decision, his payment, and profit. Since the decision was discrete in the discrete choice treatments, the results were displayed differently. The subject could see the project costs, his discrete decision (project A, project B, both projects, or none), the contribution implied by his decision, the group decision, his benefit from this group decision, his payment, and profit earned. In addition, the DCPCM treatment also displayed the decision reached by the other members of the group. This information for each project was displayed separately. All communication among subjects was prohibited during the experiment, except the transmission of responses through the computers.

2.4 Results

In discussing our results, we first present the results from the DCPCM treatment followed by a comparative evaluation of its empirical properties.

2.4.1 The DCPCM Results

Table 7 shows the frequency of dominant strategy plays in the DCPCM treatment. Overall, 72% of the time respondents behaved according to the theoretical prediction. As displayed in figure 7, unlike in the binary DCPCM study, there is a clear learning effect with percentage of dominant strategy plays increasing significantly from 60% in the first period to 85% in the 15th period ($\chi^2 = 9.40, p \leq .01$). As seen in figure 8, this learning trend among respondents is present in all sessions.

We also observe that, out of 60 participants, 12 (20%) subjects responded truthfully in all periods, and 49 (82%) subjects responded truthfully half of the time (table 8). In figure 9 and 10, we show the nature of misrevelation of demand among respondents independently for project A and project B. The horizontal axes of these figures show the range of net values (induced value-cost) given the distribution of values and costs. The vertical axes show the number of respondents who received these ranges of net values and the number of these respondents who chose that project and agreed to contribute the corresponding levels of costs for each range of net values. Perfect demand revelation implies choosing project(s) with positive net values, rejecting project(s) with negative net values, and remaining indifferent to zero net values. Figures 9 and 10 show that, similar to the binary choice study, there was both under-revelation and over-revelation of demand where some subjects were free-riding with positive net values (10%) and some were contributing with negative net values (18%) against their dominant strategies to do so.

These results reveal that the percentage of demand revelation, though not as impressive as in the binary choice case, continues to dominate the findings from the previous continuous pivotal mechanism applications. Previous applications of the pivotal mechanism found that the effectiveness of this mechanism depends heavily on how it is presented, with initial approaches finding only 8% to 50% of subjects accurately reporting their values (38; 39; 48; 49). Tideman (50), in his study of the pivotal mechanism with some college fraternities, found that 21% subjects overstating their preferences and 46% understating their preferences. However his finding is not very conclusive because he did not induce the values and also the dominant strategies were explained to the subjects. Attiyeh *et al.* (48) in their controlled laboratory experiment found that in the small group (5 person), only 10% of the time respondents were revealing their true values whereas this percentage was only 8% in the large group (10 person). They concluded that the non transparent relation between the non-equilibrium behavior and the outcomes might be a reason behind demand misrevelation. Truth revelation varied from 17% to 47% in Kawagoe and Mori (39). They offered the weak incentive compatibility of the pivotal mechanism as a possible reason behind misrevelation of demand. Cason *et al.* (38) analyzed the existence of other Nash equilibrium that differ from the dominant strategy equilibrium as a possible cause of the failure of the pivotal mechanism. Our proposed mechanism, despite having all these weaknesses achieved higher demand revelation, implying that these shortcomings are not actually the only reasons behind the misrevelation of true preferences.

2.4.2 Model Comparison

Now, we focus on the performance of the DCPCM in comparison to the alternative treatments. As indicated in table 7, demand revelation in DC-PPMBG is 78.22% compared to 72% in DCPCM, and this difference is significant at 1% (Pearson $\chi^2 = 9.32$). Moreover, there are 18 subjects who always responded truthfully in the DC-PPMBG, which is higher than the number of subjects who responded similarly in the DCPCM treatment (table 8). Contrary to the binary choice results where we have not witnessed one discrete choice treatment performing better than the other, here we find DC-PPMBG clearly outperforming DCPCM in terms of demand revelation. This is also evident from the graphical representation in figure 7, where the truth revelation trend in the DC-PPMBG lies above the same in the DCPCM in almost all periods. Looking closely at the non-equilibrium strategies we find that the percentage of over-revelation is higher in the DCPCM (18%) than in the DC-PPMBG (12%), whereas percentage of under-revelation is higher in the DC-PPMBG (12%) than in the DCPCM (10%).[9] The difference in non-dominant stragegy plays is highly statistically significant with $p < 1\%$ (Pearson $\chi^2 = 11.61$). This trend is similar to the binary choice results but the percentage of over-revelation is much severe in this case changing the overall results in favor of the DC-PPMBG. However, as expected, the demand revelation in both discrete choice treatments is higher than in the OE-PPMBG treatment, which is exhibited in figure 11.[10]

Since we started each session with the OE-PPMBG and subjects' experience levels are believed to influence their behavior, we study the effect of this experience on their demand revelations. The demand revelations under different sessions are shown in table 9, and the graphical representations of the same are given in figure 12. In sessions 1 and 2, subjects participated in the DC-PPMBG treatment first and then they participated in the DCPCM. That is, subjects participated in the DCPCM after gaining experience from the DC-PPMBG and the OE-PPMBG. Figure 12 displays the demand revelations of these two treatments for period 1 to period 15 for DC-PPMBG, and the same for DCPCM are displayed from period

[9]Note, in the DC-PPMBG free-riding is one of the multiple Nash equilibria (31; 30; 29; 27).

[10] Overall, in the OE-PPMBG treatment for multiple alternatives, the percentage of aggregate value revealed was 53.61% for project A and 52.93% for project B. On average, mean value revealed for project A was 51.86% with median at 50.70%. These values for project B were 50.72% and 51.32%, respectively. These similar value revelations for both projects are not surprising, since the induced values for both projects are drawn from the same distribution implying similar characteristics for each project. Since the objective of this paper is to demonstrate the demand revelation property of the DCPCM, we do not explicitly report the results of the OE-PPMBG.

16 to 30. The demand revelations for sessions 3 and 4 are displayed in a similar fashion. In the binary choice case, experience improved truth revelations in DCPCM, but this result does not hold for this multiple alternative choice case. In fact, when DCPCM was followed by the DC-PPMBG, as in sessions 1 and 2, the true responses decreased, but this change is not significant. On the other hand, experience increased truth revelation in DC-PPMBG, and this change in response is highly statistically significant with $p = .001$ (McNemar $chi^2 = 18$). Overall, the results indicate that DC-PPMBG performs better than the DCPCM, and this trend continues irrespective of the order.

2.4.3 The Conditional Logit Model

Next, we use a conditional logit model to investigate which treatment recovers the willingness to pay with higher accuracy. This parametric analysis also enables us to test the hypothesis that an individual's decision to accept a pre-assigned cost increases with higher induced value and falls with higher assigned cost. Following the random utility theory, an individual will choose project k from the choice set K if his utility from having this project choice gives him at least as much utility as any other choice. In this situation, we represent the probability of choosing project k by an individual i as:

$$Pr_i^k = Pr\{v_k - a_k + \epsilon_k \geq v_m - a_m + \epsilon_m; \forall m \in K\}, \tag{6}$$

where v_k is his benefit and a_k is his assigned cost for project k, and ϵ_k is the stochastic component of his utility. If the error terms are an independently, identically distributed type-I extreme value, the probability of choosing alternative k can be expressed as (64; 65):

$$Pr_i^k = \frac{\exp(v_k - a_k)}{\sum_{m \in K} \exp(v_m - a_m)}. \tag{7}$$

The estimated model parameters from both the DC-PPMBG and the DCPCM are reported in the first and second columns of table 10. We estimate three alternative specific constants by setting the choice-specific constant for alternative B to zero.[11] As expected, costs have significant negative impacts and values have significant positive impacts on choice probabilities. The alternative specific constant for project A is negative, but positive for both and neither alternatives. This implies that individuals prefer neither and both alternatives more than either alternative A or B. However, none of the alternative specific constants are statistically significant, implying the attitudinal differences among the alternatives are not significant.

The mean WTP estimates for the DCPCM and DC-PPMBG are \$12.41 and \$12.03 for project A and \$13.01 and \$12.45 for project B.[12] All these values are very close to the true mean induced value of \$12.50 for each project. These mean WTP values are double the mean WTP estimate of the open-ended treatment, where these values are \$6.48 for project A and \$6.43 for project B. Consistent with the binary choice study, once again we find that the discrete choice treatments continue to dominate the open-ended value revelation mechanism in recovering true WTP values.

Similar to the binary choice analysis, both treatments give similar coefficient estimates with similar levels of significance. However, before comparing the parameters from two different data sets, we have to isolate the scale difference (67; 53; 54).

[11]Since the alternatives are similar in characteristics, and the non-parametric test revealed similar preference for them, we decided to estimate alternative specific parameters for only one of the project alternatives.

[12]We estimate the WTP for an alternative by using the following expression (66):

$$WTP_n = \left(-\frac{1}{\beta_c}\right)\left(\ln \sum_{i\in C^0} \exp(V_{in}^1) - \ln \sum_{i\in C^1} \exp(V_{in}^0)\right),$$

where V_{in}^0 and V_{in}^1 represent the utility individual derives before and after the change from alternative i, respectively.

Since it is not possible to identify the scale factor for both data sets, we estimate a relative scale factor by normalizing one scale factor and allowing the other scale factor to vary. We estimate a conditional logit model by pooling the data sets and restricting the parameters from both data sets to be equal. The results from the pooled model are given in the third column of table 10. A log-likelihood test statistic rejects the equal parameter hypothesis ($p < .001$).[13] This result is consistent with the binary choice results, and once again implies that although our econometric analysis yields similar welfare measure under both discrete choice treatments, it does not indicate identical preference pattern for individuals.

2.5 Discussion

In our previous paper Das and Anderson (61), we discussed a dominant strategy incentive compatible mechanism design for stated choice surveys. To do this, we adapted Clarke's pivotal mechanism to a binary choice case, where an individual's task was to decide whether or not to contribute a fixed amount toward the provision of a fixed-size public good. We presented a formal proof of incentive compatibility of the mechanism and designed an induced value experiment to validate the theoretical prediction. Our experimental results showed that the DCPCM, though not perfectly demand revealing, performed quite well. Overall, even though DCPCM performed similarly to the discrete choice PPMBG in terms of both truth revelation and mean WTP estimates, econometric analysis revealed that these two alternative demand revelation methods do not imply similar behavioral pattern for individuals.

In this paper, we extend this mechanism design for a multiple alternative choice case. We present a formal proof of incentive compatibility of the proposed

[13] To test the null hypothesis that DC-PPMBG and DCPCM have the same parameters, we calculate the test statistics as $LLR = -2[(L^{PPMBG} + L^{DCPCM}) - L^{Pooled}]$, which follows a chi-square distribution with |no of common parameters|-1 degrees of freedom (53). The test statistics for the equal parameter hypothesis is $-2(1629.713 - 742.892 - 873.349) = 26.94 \sim \chi^2_4$

mechanism and use experimental tools to verify the dominant strategy equilibrium property of the mechanism. For theoretical formulation and experimental illustration, we restrict our choice set to four discrete choice alternatives. We also evaluate the performance of this mechanism with respect to an open-ended PPMBG mechanism and a discrete-choice PPMBG mechanism.

The multiple choice analysis reveals that, consistent with the binary choice analysis, the discrete choice treatments perform better than the open-ended treatment both in terms of truth revelation and mean WTP estimates. However, in the multiple choice case, the DC-PPMBG clearly perform better than the DCPCM mechanism. This result is different from the binary choice result, where we found no clear indication of one discrete choice treatment performing better than the other. Again, contrary to the binary choice analysis, we perceive a clear learning trend among respondents in this multiple alternative choice case.

The low percentage of truth revelations in the first period and overall, raises a question regarding the effectiveness of the DCPCM for the multiple alternative choice case. We argue that in the pivotal mechanism, there are two factors that respondents have to understand to identify their dominant strategies: the definition of pivotal condition and the definition of pivotal tax. Intuitively, even if it is possible for the respondents to comprehend the concept of pivotal condition, it might be difficult for them to make a mental calculation of the possible pivotal tax that their decisions imply, or it might not be immediately clear to them exactly how much they are pledging toward the public good provision. Kawagoe and Mori (39) also discussed the confusion regarding the calculation of Clarke tax as a possible cause of the failure of the pivotal mechanism. Our DCPCM simplifies the mechanism immensely by removing the complications associated with the pivotal tax definition. This was reflected in the high percentage of truth revelation in the binary choice case. However, the multiple alternative choice case reintroduces some of these complications through the different pivotal condition definitions resulting in low demand revelation. These might also be the reasons behind the high percentage of demand over-revelation.

This misrevelation of demand can also be a result of altruism, warm-glow, and confusion. Researches agree that altruism, warm-glow and confusion are prevalent in laboratory experiment and often are reasons behind the failure in attaining the equilibrium. In public good contribution game, people often tend to contribute because they receive utility from the act of giving (warm glow) or when their contributions lead to an increase in the others' payoffs (altruism). Recently, several researchers studied these phenomenon in public good games. For example, Palfrey and Prisbrey (55) found evidence of warm glow but rejected the presence of altruism. On the other hand, Anderson *et al.* (56) and Goeree *et al.* (57) found presence of altruism. Andreoni (58) suggested that confusion might also be responsible behind non-dominant strategy plays. Besides, Sagoff (59) advocated the idea that, while making a social choice, an individuals might play the role of a citizen who cares more about society, than an individual who pursues self interest. Therefore, he might contribute out of moral obligation toward the society, even though it is against his own benefit. Nyborg (60) presented a formal model to discuss the implication for environmental valuation if individuals take two distinct roles, as a consumer and as a citizen. However, since this area of research is almost unexplored, there is a lack of evidence whether such multiple preference ordering exists.

Overall, although the DCPCM for multiple choice did not perform as well as the DC-PPMBG or the DCPCM for the binary choice, even with four alternatives the truth revelation was higher when compared to the same from previous pivotal mechanism applications (49). From a policy point of view, this analysis suggests that the DCPCM has potential to perform successfully, but more research is needed before it can be applied in the field to solve real world resource allocation problems.

Table 6. Treatment orders

Session	Treatment1	Treatment2	Treatment3	Treatment4
1	OE-PPMBG(binary)	OE-PPMBG	DC-PPMBG	DCPCM
2	OE-PPMBG(binary)	OE-PPMBG	DC-PPMBG	DCPCM
3	OE-PPMBG(binary)	OE-PPMBG	DCPCM	DC-PPMBG
4	OE-PPMBG(binary)	OE-PPMBG	DCPCM	DC-PPMBG

Table 7. Truth revelation in the discrete choice questions

Treatment	Number of true responses	Percentage of true responses	Total
DCPCM	648	72.00%	900
DC-PPMBG	705	78.22%	900

Table 8. Comparison of the number of subjects always responding truthfully under the discrete choice treatments

Treatment	Number of subjects always telling the truth	Number of subjects not always telling the truth	Total responses
DCPCM	12	48	60
DC-PPMBG	18	42	60

Table 9. Order effect on truth revelation

Treatment	Number of true responses	Percentage of true responses
DC-PPMBG (without experience: session 1-2)	342	76.00%
DC-PPMBG (with experience: session 3-4)	362	80.44%
DCPCM (with experience: session 1-2)	340	75.56%
DCPCM (without experience: session 3-4)	308	68.45%

Table 10. Conditional logit model estimates

Variable	Coefficient estimates		
	DC-PPMBG	DCPCM	Pooled model
constantA	-0.080	-0.067	-0.086
	(0.608)	(-0.543)	(-0.846)
constantBoth	0.022	0.092	0.054
	(0.104)	(0.491)	(0.340)
constantNone	0.030	0.297	0.1835
	(0.141)	(1.479)	(1.116)
value	0.603	0.479	0.614
	(19.078)	(17.718)	(20.149)
cost	-0.612	-0.450	-0.602
	(21.659)	(20.563)	(-21.364)
scale			0.744
			(14.726)
Log-likelihood	-742.892	-873.349	-1629.713

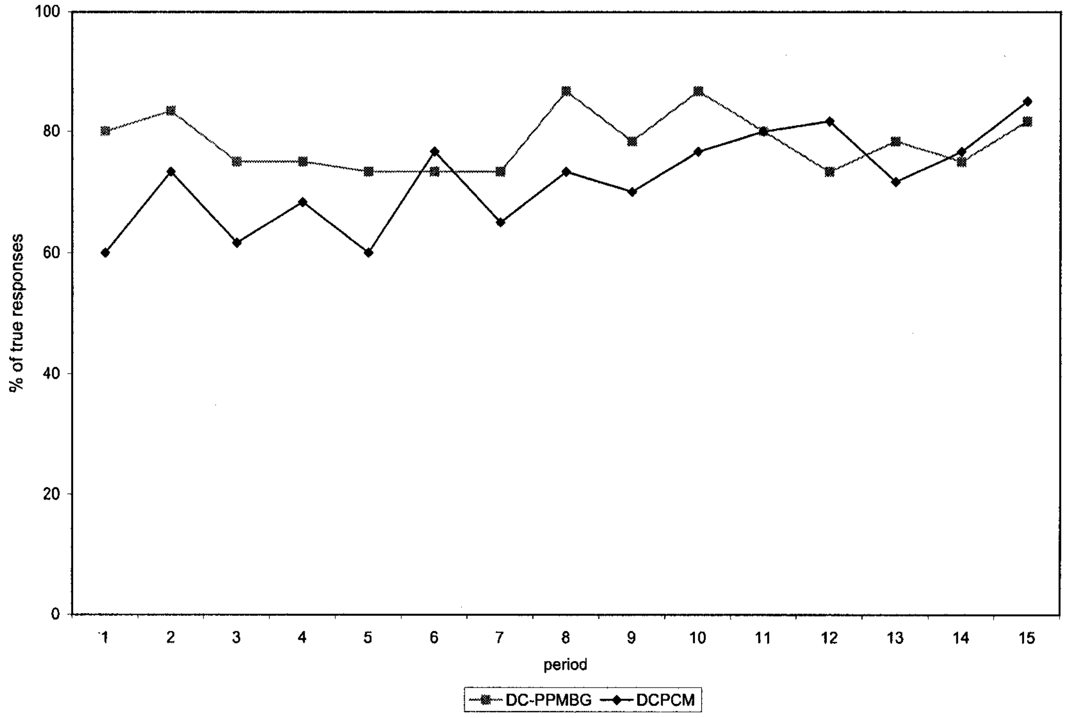

Figure 7. Percentage of truth revelation in the DC-PPMBG and the DCPCM treatments

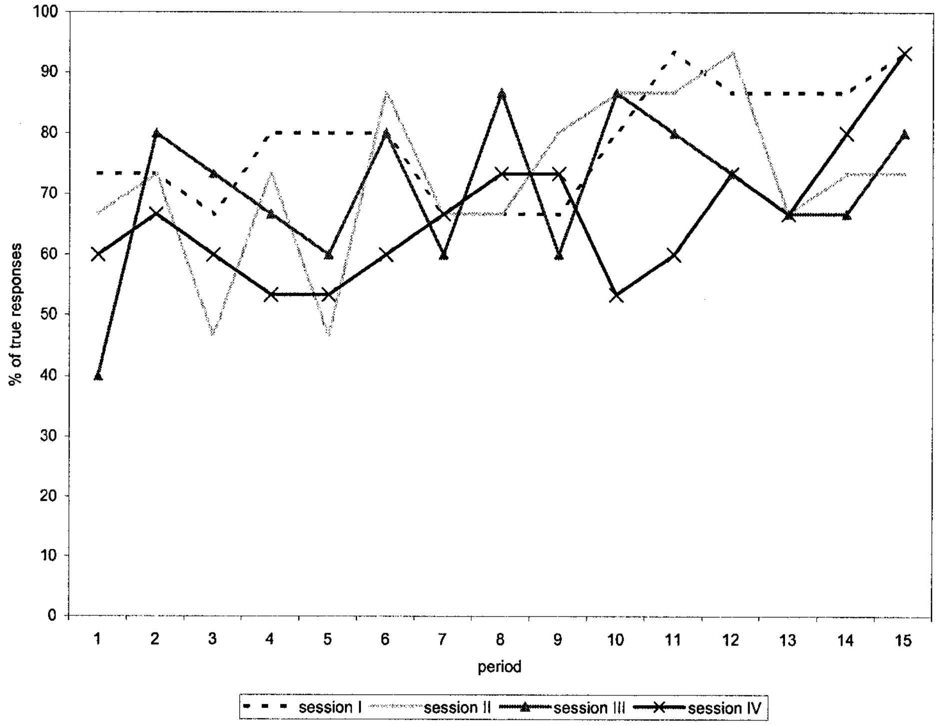

Figure 8. Percentage of truth revelation in the DCPCM by session

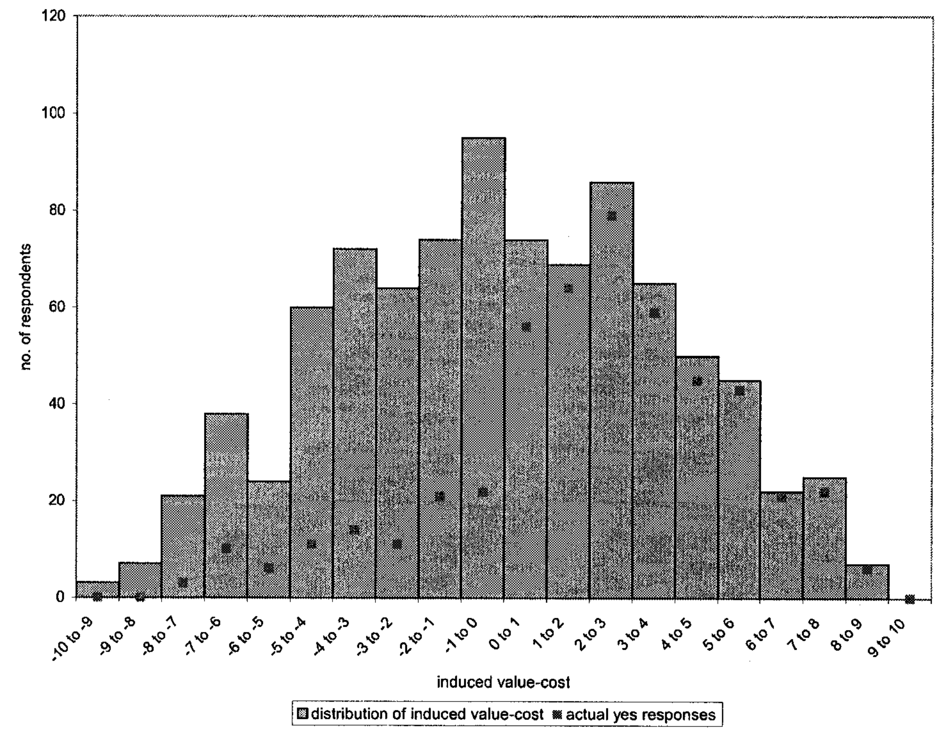

Figure 9. Nature of truth revelation in the DCPCM for project A

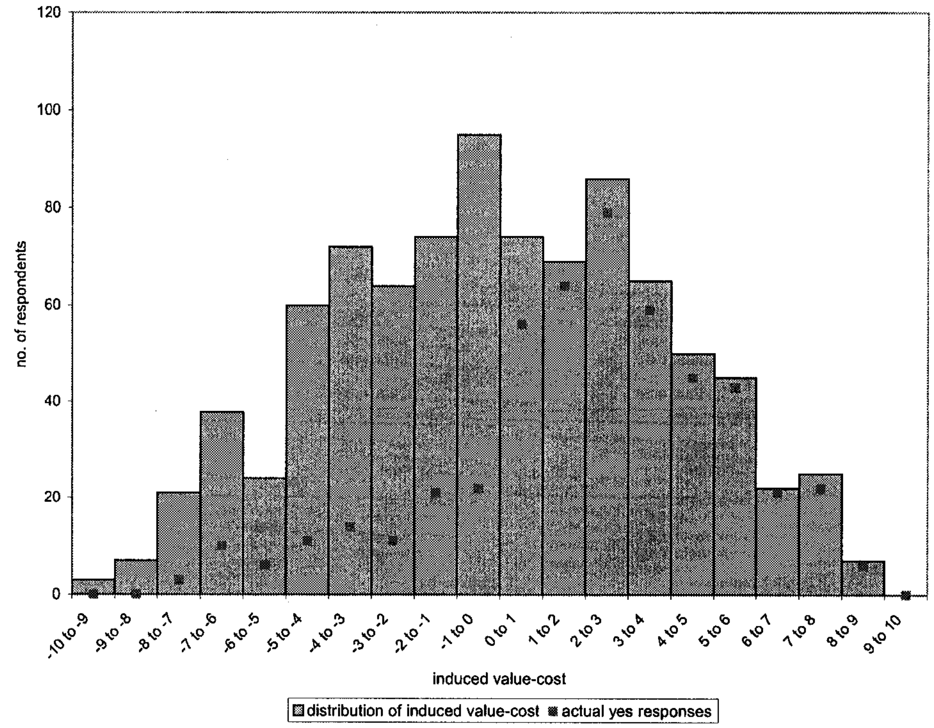

Figure 10. Nature of truth revelation in the DCPCM for project B

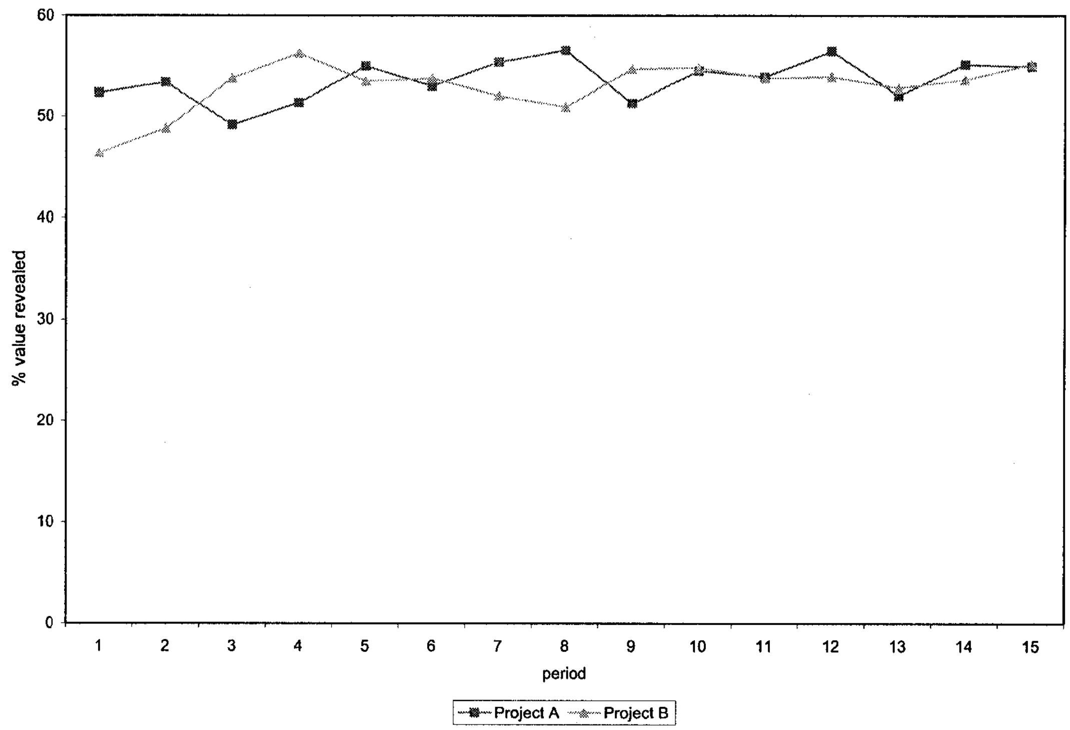

Figure 11. Average percentage of value revealed in the OE-PPMBG

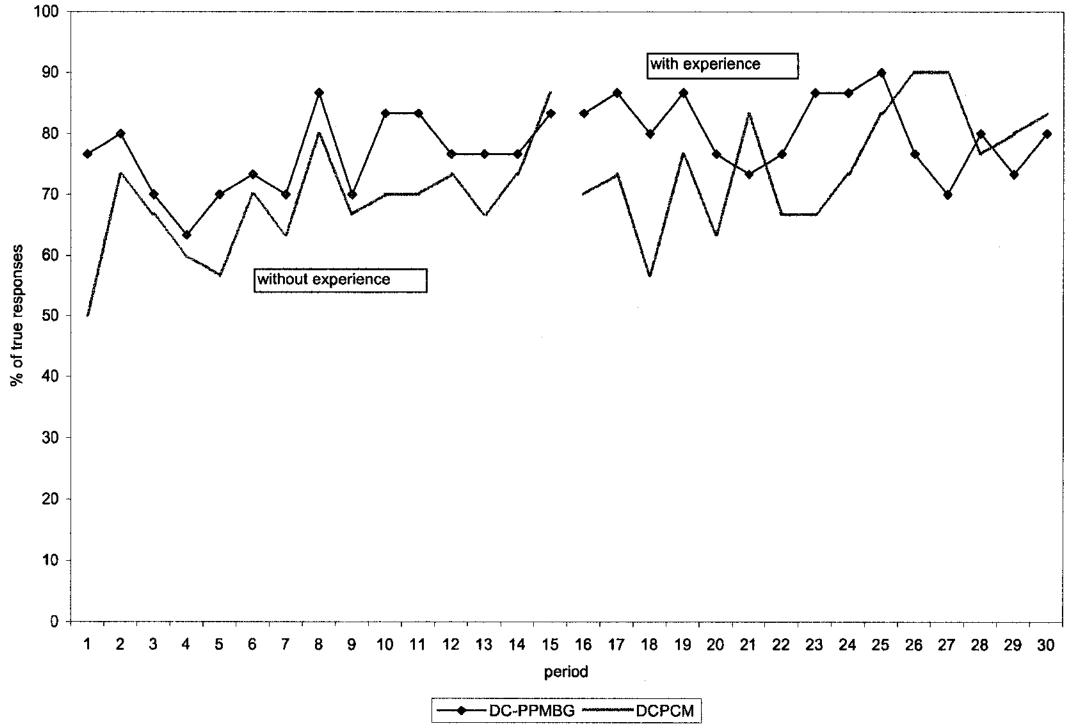

Figure 12. Robustness test of the discrete choice treatments

MANUSCRIPT 3

Direct Estimation of Distributions of Willingness to Pay for Heterogeneous Populations

Abstract

Random parameters logit models have emerged as a standard approach to obtain efficient estimates from discrete choice surveys designed to elicit public preferences over nonmarket environmental amenities; counterfactual transportation and tourism alternatives; and marketing and public health care alternatives. However, to simplify calculation of welfare measures, the marginal utility of income is often assumed to be constant across the population. We show that, by shifting distributional assumptions from marginal utilities to the welfare measures themselves, a random parameters version of a censored logistic regression model (68) directly yields distributions of welfare measures without imposing any unnecessary assumption on marginal utility of income. We develop the theoretical framework for the model, and apply it to contingent choice survey data for siting a noxious facility in Rhode Island. In this application, our proposed model and random parameters logit yield similar mean willingness to pay, but welfare measures are more readily interpretable in the proposed model.

3.1 Introduction

Discrete choice methods have been used by many disciplines to evaluate trade-offs presented by policy alternatives. Examples include choice among development restrictions to preserve different amenities provided by open space; alternative groundwater pollution regulations; alternative locations for noxious or dangerous facilities such as landfills or liquefied natural gas terminals; alternative modes or routes for transportations; alternative product brands and health care programs. The theoretical underpinning of discrete choice method is the principle of utility maximization. The analysis proceeds by presenting the decision makers choices between alternative outcomes. Each respondent selects the outcome with the attribute combination she most prefers, stating her preferences for the trade offs implicit among the listed attributes of alternative outcomes. Given a large number of responses, a statistical discrete choice model is used to recover the relative weight each attribute is given in the utility function of the respondent. This utility function can, in turn, be used to understand how alternative policies affect the welfare of the sampled population.

Examples of discrete choice applications can be found in the field of environmental application, transportation, marketing and public health. Published studies have used this approach to, among many other purposes, value impacts of noxious facilities (69), water quality (46), assess consumer preferences for medical providers (70) and alternative transportation modes (71). These applications use random utility-based models, such as logit or probit, to estimate utility as a function of the attributes describing the alternative outcomes, based on the observed discrete choices. The coefficient associated with each attribute is the weight given to that attribute in a representative agent's utility function. It can be monetized by dividing by the marginal utility of income (72), yielding a willingness to pay or willingness to accept for changes in an attribute level. Policy makers may use these values to determine efficient management decisions.

More recent discrete choice analysis has moved away from straightforward discrete choice models to models that recognize the heterogeneity of preferences over choice alternative attributes. Such models offer greater statistical efficiency and information about the distributional effects of policies, which may be of interest to political decision makers. Several approaches have been taken by researchers to incorporate preference heterogeneity into their analysis. Boxall and Adamowicz (73) use a latent class model to value the recreational demand for wilderness parks. Breffle and Morey (74) specify different parametric methods to model heterogeneity in the choice of sites for salmon fishing. These methods include modeling utility as a function of individual characteristics, a random parameters model, and an utility function with a heterogeneous scale parameter.

A commonly adopted approach is random parameters logit, or random coefficients logit. Random parameters logit (RPL) explicitly accounts for heterogeneity by allowing each individual to have a different preference pattern; instead of estimating a common set of attribute marginal utilities for all respondents, each respondent has his or her own marginal utilities, and the distributions of marginal utilities in the population are estimated. This approach has become popular for several reasons. First, RPL removes three limitations of standard logit by allowing heterogeneity in preferences, unrestricted substitution patterns in multinomial choice situations, and correlations in unobserved factors over time. Second, it is also highly flexible so that it can approximate any random utility model ((3), Chapter 6). Finally, compared to logit models with homogeneous parameters, RPL not only gives efficient parameter estimates, but also gives distributions of preference measures rather than a single preference measure for a representative individual.

The RPL applications include both stated preference and revealed preference data. Revelt and Train (75) estimated a random parameters logit for households' choices of appliances with repeated choices. Layton and Brown (76) examined the heterogeneity of preferences in mitigating climate change impacts. Morey and Rossman (77) used RPL to model the preservation of marble monuments in Washington DC. Train (78) modeled a random parameters logit of anglers' choices of fishing sites. Nahuelhual *et al.* (79) studied heterogeneity of preferences for protection of public open space. Brownstone *et al.* (80) estimated a mixed logit model merging both stated preference and revealed preference data for alternative-fuel vehicles. Rouwendel and Meijer (81)estimated a mixed logit model to analyze the preferences of workers with respect to housing, job and commuting. Anderson *et al.* (82) modeled a random parameter logit of parking preferences among tourists. Bhat and Sardesai (83)used RPL to model commuter's mode choice using both revealed preference and stated preference data. Hall *et al.* (84) estimated a multinomial logit model with random coefficients to determine the factors that influences consumer's preferences for genetic carrier testing. Many more applications can be found in the literature.

In standard application, RPL yields distributions of marginal utilities, leaving analysts to produce willingness to pay distributions by dividing the distributions of attribute coefficients by the distribution of the alternative cost coefficient. However, calculating a ratio of two distributions can be difficult, especially if the distribution of the denominator, the cost coefficient in this case, has mass close to zero, and can lead to high means and WTP distributions with very thick tails. To address this problem, the RPL model is often specified with a fixed (non-random) cost coefficient; it is straightforward to divide the attribute distribution by a constant to obtain a distribution of WTP.[1]

[1]In some instances, cost is assumed fixed because of identification or convergence problems

An alternative way to avoid the complications regarding the distributional assumption of the cost coefficient is to impose the heterogeneity structure directly on the WTP rather than on the utility coefficients. This approach adapts Cameron's (68) censored logistic regression (CLR) model, which estimates a random expenditure function normalizing the cost coefficient to one (since the marginal expenditure of a dollar spent is a dollar for everyone) and instead estimates the logit scale parameter. While this "new paradigm" presented a different way to think about welfare measure estimation, few were willing to hand-code likelihood functions to achieve direct measures which are readily available by transforming results from canned logit routines.

However, random parameters models significantly complicate the transformation necessary to get WTPs, warranting a reconsideration of Cameron's approach. We show that a random parameters version of censored logistic regression yields distributions of willingness to pay for outcome attributes, which facilitate policy formulation more directly and efficiently, and require no complicated transformation as in RPL. However, which predicts better is an empirical question, and that the latter provides easier interpretation may dominate economically unimportant differences in fit. The point of this paper is to demonstrate this alternative methodology, and point out its advantages in a sample environmental application.[2]

Concurrent with this research, two equivalent models for directly estimating distributions of willingness to pay have been developed. Sonnier *et al.* (86) use Bayesian methods to analyze automobile choice as a function of attributes and branding using a random parameters logit model, with the price coefficient normalized to 1 and an estimated scale parameter. In a Monte Carlo analysis resampling from their actual survey design and computing choices based on assumed WTP

(75; 85).

[2]Establishing the systematic empirical superiority of one model or the other requires comparison across many data sets, a task beyond the scope of the current paper.

distributions, they find RPL actually fits best, but that direct distribution estimation yields better true WTP recovery; on their actual response data, the WTP model had lower in-sample log-likelihood, but higher out-of-sample log-likelihood. Train and Weeks (87) use Bayesian methods to estimate the algebraically equivalent reciprocal of the scale parameter as a lognormally distributed price coefficient, which also multiplies the willingness to pay distribution for each attribute, in an analysis of preferences for alternative-fuel cars. They find that RPL fits their data better than estimating distributions of willingness to pay, but yields incredibly large WTP values; the directly estimated WTP distributions are more sensible. Our paper complements this analysis in three ways. First, it is the first to adopt a classical maximum-likelihood estimation approach in demonstrating the advantages of direct estimation of WTP distributions. Second, our model presentation is based on Cameron's CLR model. Basing our exposition in a familiar model and common estimation paradigm clarifies the similarities, and differences introduced by the random parameters implementation, with familiar models. Third, our empirical analysis adds to collective body of evidence on the use and interpretation of models directly estimating distributions of willingness to pay.

The next section presents the theoretical framework of the model. Section 3 describes the Rhode Island landfill siting application we use to demonstrate the model, and section 4 discusses the results and how they improve upon the homogeneous and RPL results for policy purposes. Section 5 concludes.

3.2 The Theoretical Framework

We develop our model within a standard contingent choice environment where an individual makes a choice among K hypothetical alternatives, based upon the alternative attributes and the cost of provision.

3.2.1 Estimating Distributions of Marginal Utilities

Random parameters logit is based on a random utility model, and thus estimates distributions of attribute marginal utilities. In this model, the utility obtained by individual i from alternative j is given by

$$U_{ij} = x_{ij}\beta_i - c_{ij}\beta_{ci} + u_{ij}, \tag{8}$$

where c_{ij} is the cost to i of alternative j, β_{ci} is i's cost coefficient, x_{ij} is the vector of non-cost attributes for alternative j and β_i is the vector of attribute coefficients, which varies randomly in the population. Within the random utility framework, the coefficients are interpreted as marginal utilities, and β_{ci} is the marginal utility of income. The error term u_{ij} captures components of i's utility from j which are not observable to the investigator.

Since the dependent variable U_{ij} is unobservable, a preference for an alternative j is manifested through the discrete choice vector d_{ij}, such that

$$d_{ij} = \begin{cases} 1 & \text{if } U_{ij} > U_{ik} \ \forall\ k \neq j \\ 0 & \text{otherwise.} \end{cases}$$

Since d_{ij} is a function of the unobservable u_{ij}s, i's choice is random to the investigator. The probability that individual i chooses alternative j is

$$\begin{aligned} Pr(d_{ij}=1) &= Pr(x_{ij}\beta_i - c_{ij}\beta_{ci} + u_{ij} > x_{ik}\beta_i - c_{ik}\beta_{ci} + u_{ik} \ \forall\ k \neq j), \\ &= Pr((x_{ij}-x_{ik})\beta_i - (c_{ij}-c_{ik})\beta_{ci} > u_{ik} - u_{ij} \ \forall\ k \neq j). \end{aligned} \tag{9}$$

The variance of right-hand error term can be normalized by introducing a scale parameter, σ, which captures the variance of the error distribution,

$$Pr(d_{ij}=1) = Pr((x_{ij}-x_{ik})\tfrac{\beta_i}{\sigma} - (c_{ij}-c_{ik})\tfrac{\beta_{ci}}{\sigma} > \tfrac{(u_{ik}-u_{ij})}{\sigma} \ \forall k \neq j). \tag{10}$$

However, the scale parameter and full set of β coefficients are not separately identifiable, as every β is divided by σ. The standard logit and RPL models normalize the scale to one and estimate attribute coefficients that are the ratio of the $\frac{\beta}{\sigma}$ values under a standard logistic distribution for $\frac{(u_{ik}-u_{ij})}{\sigma}$ with mean zero and variance $\frac{\pi^2}{6}$. Since the variance of the error term can be different for different individuals, and ignoring this might results in inaccurate interpretation, the scale is considered to be random. This yields a RPL log-likelihood function

$$LL(\theta) = \sum_I \ln \int \prod_j \left[\frac{\exp(x_{ij}(\frac{\beta_i}{\sigma_i}) - c_{ij}(\frac{\beta_{ci}}{\sigma_i}))}{\sum_k \exp(x_{ik}(\frac{\beta_i}{\sigma_i}) - c_{ik}(\frac{\beta_{ci}}{\sigma_i}))} \right]^{d_{ij}} f\left(\frac{\beta}{\sigma};\theta\right) d\left(\frac{\beta}{\sigma}\right), \quad (11)$$

where $f(\frac{\beta}{\sigma};\theta)$ is the, possibly joint, density of the $\frac{\beta}{\sigma}$ ratios and θ is the estimated parameters of that density. In the absence of systematic research guiding the selection of distributions, most authors have specified $f(.)$ to be a set of independent normal or lognormal distributions, with θ representing their means and variances.[3]

The model yields distributions of relative marginal utilities for each attribute, not identified separately from their scale. The willingness to pay measure of welfare for person i is given by the ratio of i's estimated coefficient for attribute ℓ, $\frac{\beta_{\ell i}}{\sigma_i}$ and i's marginal utility of income, $\frac{\beta_{ci}}{\sigma_i}$; the unidentified scale parameter which makes these marginal utilities relative and divides both β coefficients cancels in the ratio, yielding an absolute dollar-denominated measure, $\frac{\beta_{\ell i}}{\beta_{ci}}$. When both the attribute and cost coefficients are random, the distribution of willingness to pay is the ratio of the distributions, which can be obtained by taking the ratio of a large number of draws from each distribution. However, this computation can be cumbersome, and may yield nonstandard distributions or distributions with incredibly thick tails when the cost coefficient distribution is nonnormal (e.g. lognormal) and has much mass near zero (e.g., Train and Weeks, (87)). To avoid these problems, analysts often assume the cost coefficient is non-random. If the cost coefficient is fixed, then the WTP follows the same distribution as the attribute coefficient, and the

[3]Note that the integral in equation 11 does not have a closed form solution for most distributions $f(.)$. It can be integrated numerically, and estimation is done through maximizing the simulated log-likelihood.

mean WTP for a particular attribute is calculated as the ratio of the mean of the random coefficient and the fixed price coefficient. The standard deviation of WTP is calculated as the ratio between the standard deviation of the random coefficient and the price coefficient. While simplifying the WTP calculation, this approach compromises some of the statistical advantages that the RPL model was intended to achieve over the homogeneous logit model.

3.2.2 Estimating Distributions of Willingness to Pay

An alternative approach to estimating distributions of willingness to pay can be accomplished by changing the normalization used in estimating the random utility model. Cameron and James (88) and Cameron (68) argue that if the cost coefficient is normalized to one and the scale parameter is estimated, the estimated utility function becomes a money-scaled expenditure function, with attribute coefficients directly yielding marginal WTP values. Here we extend this censored logistic regression model, so-called to distinguish it from scale-normalized logit, by introducing heterogeneity through random coefficients, and call it random parameters censored logistic regression (RPCLR). RPCLR yields well-behaved, readily available distributions of WTP without any arbitrary assumptions about the cost coefficient.

In the censored logistic regression formulation, the dependent variable, Y, is the WTP for the alternative with attributes x_{ij}, rather than utility as in the random utility model; cost enters the model separately, and not as an explanatory variable. The valuation function is

$$Y_{ij} = x_{ij}\beta_i + u_{ij}. \tag{12}$$

In this scenario, an individual i will choose an alternative j if her net willingness to pay for alternative j, defined as the difference between Y_{ij} and cost, exceeds that of the other available alternatives. Therefore the choice indicator variable is given by

$$d_{ij} = \begin{cases} 1 & \text{if } (Y_{ij} - c_{ij}) > (Y_{ik} - c_{ik}) \ \forall\, k \neq j \\ 0 & \text{otherwise,} \end{cases}$$

where c_{ij} is the cost of alternative j to individual i.

The probability that person i will choose alternative j is given by

$$\begin{aligned} Pr(d_{ij} = 1) &= Pr(Y_{ij} - c_{ij} > Y_{ik} - c_{ik} \ \forall k \neq j), \\ &= Pr((x_{ij}\beta_i - c_{ij}) - (x_{ik}\beta_i - c_{ik}) > (u_{ik} - u_{ij}) \ \forall k \neq j). \end{aligned} \tag{13}$$

As in the RPL specification, the variance of the right-hand term can be normalized by dividing by a scale parameter,

$$\begin{aligned} Pr(d_{ij} = 1) &= Pr((x_{ij}\beta_i - x_{ik}\beta_i)/\sigma_i - (c_{ij} - c_{ik})/\sigma_i > (u_{ik} - u_{ij})/\sigma_i \ \forall k \neq j), \\ &= Pr((x_{ij} - x_{ik})\tfrac{\beta_i}{\sigma_i} - (c_{ij} - c_{ik})\tfrac{1}{\sigma_i} > \tfrac{(u_{ik} - u_{ij})}{\sigma_i} \ \forall k \neq j). \end{aligned} \tag{14}$$

Because there is no β coefficient on the c_{ij} cost terms, it is possible to identify the scale parameter and the β_is separately. Using a logit formulation, the choice probabilities can be estimated under a standard extreme value distribution with variance $\frac{\pi^2}{6}$. This yields an RPCLR log-likelihood function

$$LL(\theta) = \sum_I \ln \int \prod_j \left[\frac{\exp(x_{ij}(\frac{\beta_i}{\sigma_i}) - c_{ij}\frac{1}{\sigma_i})}{\sum_k \exp(x_{ik}(\frac{\beta_i}{\sigma_i}) - c_{ik}\frac{1}{\sigma_i})} \right]^{d_{ij}} g(\beta, \sigma; \theta) d(\beta, \sigma), \tag{15}$$

where $g(\beta, \sigma; \theta)$ is the, possibly joint, density of the β and σ, and θ is the parameter vector of that density. Because the attribute coefficients are scaled by an identified scale parameter, the estimated βs are scaled relative to a cost coefficient of one. Thus, the resulting coefficients are direct estimates of willingness to pay, and $g(\beta, \sigma; \theta)$ is the distributions of willingness to pay in the sampled population.[4]

[4] Sonnier *et al.* (86) use Bayesian methods to estimate a similar model, where they also identify the scale parameter by normalizing the price coefficient and interpret the attribute coefficients as willingness to pay distributions. Train and Weeks (2005) use Bayesian methods to estimate an algebraically equivalent model, interpreting $\frac{1}{\sigma}$ as the "price coefficient," which also multiplies the attribute β_is, which they also interpret as a distribution of willingness to pay.

These estimates, and their corresponding standard errors, can be used directly in welfare analysis.[5]

There is one important difference between the renormalization applied to derive censored logistic regression and applying that same renormalization in the random parameters environment. Cameron (68) shows that in the representative agent framework, logit and censored logistic regression are simply transformations of one-another: the logit parameters can be recovered from the censored logistic regression parameters through a simple transformation (divide estimated WTPs by the estimated scale parameter). This is not the case for RPCLR. The reason is that the distributional assumptions are over different measures, scaled marginal utilities in RPL and willingness to pay in RPCLR. The resulting nonlinear relationships do not allow exact recovery of one distribution from the other because, for example, a distribution of willingness to pay for an attribute is often calculated as the ratio of a normal attribute coefficient distribution and a lognormal cost coefficient distribution, resulting in a nonstandard distribution. In estimating willingness to pay directly, a researcher would likely assume the corresponding attribute willingness to pay would be normally distributed, rather than being drawn from a ratio of normal and lognormal deviates.

From a practical standpoint, there is little reason to believe that marginal utilities necessarily have standard distributions (such as normal and lognormal) and willingness to pay have the nonstandard distributions implied by that specification, rather than willingness to pay having standard distributions and marginal utilities have the nonstandard distributions implied by computing them based on estimated standard distributions of WTP.[6] One response to this uncertainty is that

[5]Note that equation 15 has one fewer parameter than equation 11. However, standard practice is to not estimate a population variance for β_{ci} in the RPL model, yielding comparable models with identical numbers of coefficients.

[6]We suspect the common practice of estimating distributions of marginal utilities has simply arisen as historical accident through directly applying random parameters techniques to the

determining which model is correct is simply an empirical question, and the approach that fits better in a particular application is the appropriate one. We argue that in the absence of strong evidence that marginal utilities systematically have standard distributions and WTPs do not, that RPCLR presents an alternative theoretical framework within which it is possible to directly estimate distributions of willingness to pay which are well-behaved and have any standard form, and to do so without needing to restrict any likely random parameters to yield tractable or credible results.

In the next section, we demonstrate the tractability of the RPCLR model by applying it to a contingent choice dataset. We compare our results for estimated WTPs to those from a CLR, and to RPL. While all models yield similar WTP values, and the RPCLR specification actually fits better than RPL in-sample but as well as RPL out of sample in this application, we emphasize the advantage of the RPCLR perspective: that we are able to obtain efficient and easily interpretable welfare estimates for the same number of parameters as a RPL model with a fixed-value cost coefficient. Further, we find that estimating a joint distribution, with correlations among attribute tastes, yields results that are economically different than results obtained by assuming WTP distributions are independent across attributes.

3.3 An Empirical Illustration

To demonstrate the RPCLR model, and to gain an initial sense of the empirical significance of assuming distributional forms on willingness to pay rather than marginal utilities, we estimate a simple linear valuation function for landfill sites using contingent choice survey data drawn from a study of Rhode Island residents.

standard logit-based analysis of contingent choice data. Had censored logistic regression caught on when it was introduced, the approach we are proposing would likely be the common practice. Until now, it has not been observed that the goal of estimating distributions of willingness to pay presents an affirmative reason to switch perspectives.

The Rhode Island data was previously used in studies of noxious facility siting (69; 89; 90), studies of modeling heterogeneous preferences (91), testing neo-classical theory (92) and in alternative interpretation of preference indicators (93).

In our application, each survey question asked respondents to choose between two alternative landfill locations. Alternative locations were described in terms of attributes of a fixed 500 acres parcel to be converted to a landfill, and attributes of the surrounding one mile radius, which might be affected by landfill traffic and pollution. The site itself was described in terms of the acres of farmland (*Farms*), marshland (*Marsh*) and forestland directly converted to landfill;[7] the quality of the groundwater in the area (*Groundwater*: 1=high and 0=low); and the quality of the wildlife habitat in the area (*Wildlife: 1=unique habitat and 0=normal habitat*). The location attributes include the number of houses within a one mile radius (*Homes*); the presence of parkland (*Park: 1=present, 0=absent*) and farmland (*Farmland: 1=present, 0=absent*); the presence of schools (*School: 1=present, 0=absent*); and the nature of access roads (*Highway: 1=access by highway, 0=access by local roads*). The cost is a continuous variable indicating the annual average cost per household (*Cost*), paid in additional annual state taxes to cover landfill construction and maintenance. Table 11 summarizes definitions of the variables used in the empirical analysis.

The survey was administered in person at public locations (e.g., malls, RI Department of Motor Vehicles offices and other public gathering places) to a total of 1,151 residents, yielding approximately 40 replications of each of 28 distinct 10-question survey booklets. 44 residents, out of 1151, did not provide residency or reported residency outside Rhode Island, therefore their responses were excluded from the sample. This analysis uses 10,703 responses from 1107 people. Choices are

[7]The land type forest is excluded because the acreage of the three types always sums to the landfill size, 500 acres.

analyzed as a function of cost, the site and location attributes, and each attribute interacted with an indicator for whether the respondent was from Providence, since development patterns made it much less likely a landfill would directly affect Providence residents.[8]

3.3.1 Estimation Results

Using the landfill data, we calculate willingness to pay measures for landfill site attributes using four econometric specifications: a fixed-parameters CLR, a RPL, RPCLR with attribute coefficients assumed to be independent and normally distributed, and RPCLR with multivariate normal attribute coefficients.[9] In the random parameters models, the scale parameter is assumed to follow a lognormal distribution, and the Providence resident interaction variables are treated as fixed mean shifters.[10,11]

The results are presented in Table 12. The second column reports coefficients of the CLR, which give the marginal willingness to pay for each attribute by a representative agent, and which are equivalent to the willingness to pay that would be calculated from a standard logit model. A negative coefficient implies

[8]The original model of Opaluch *et al.* (69) included 194 variables, largely interactions of attributes with respondent demographics. Such a large model would be unwieldy in a random parameter framework. Comparison of the two results can suggest whether random coefficient models capture heterogeneity in essentially the same way as demographic interactions within a standard logit framework. Our specification comes from Swallow *et al.*'(93) resampling-based analysis of the same data. We expect the random parameters to pick up this observed and unobserved heterogeneity. It is possible to specify the mean and variance of the coefficient distributions in random parameters logit model as functions of demographic characteristics or other exogenous factors, which might capture additional structure in this heterogeneity.

[9]The literature provides little guidance on selection of distributional families for attribute coefficients. We selected normal distributions because they are a standard choice, and because stories can be told for each attribute where respondents may have a positive or negative WTP. For our application, individuals might have either positive or negative attitude towards the attribute variables. Therefore these attribute coefficients are assumed to follow a normal distribution.

[10]A log-normal distribution is chosen for the scale parameter to restrict $\sigma > 0$. In RPL, for identification, the median for log-normally distributed scale is fixed at one by setting the mean of $ln(\sigma)$ equal to zero (74).

[11]All the models are estimated using GAUSS. For both RPL and RPCLR, 200 Halton draws (94) are used and they converged under 60 minutes from maxima of preliminary estimates. 500 Halton draws are used for the RPCLR with correlation, which took almost 11 hours to converge.

a marginal decrease in money-normalized utility resulting from an increase in the corresponding attribute level, or a positive willingness to pay to avoid increasing that attribute at or near the landfill site. For site attributes, the results indicate a willingness to pay of \$248.98 to avoid disturbing unique wildlife habitat, and more than twice that, \$556.89, to avoid areas with high quality groundwater. Among land types, residents least prefer to convert farmland, which they value at \$1.39 more per acre than the excluded forestland, with marshland falling between the two with a value of 55 cents per acre more than forestland. For the surrounding area, residents most prefer to avoid schools, with a willingness to pay of \$367.97 to move to an equivalent site without a school, followed by farms, public parks and scattered homes. Residents are willing to pay about \$280 to keep garbage trucks off local roads, instead accessing the landfill from major highways. Compared to suburban and rural residents, Providence residents prefer converting marshland to converting forestland, mind proximity to farmlands \$81.97 less, and are willing to pay significantly less, only \$120, for highway rather than local road access.

The third column of Table 12 reports the estimated attribute means from RPCLR, and the fourth column the attribute standard deviations. Overall, mean estimated willingness to pay for attributes are remarkably similar to those based on CLR; the relative priorities of each attribute remain the same. However, the willingness to pay estimates tend to have higher absolute values. This is a common result in random parameters models as variation which is unexplained in the fixed parameters model, and is thus assigned to the scale parameter which divides coefficient values, is instead associated with population heterogeneity on specific attributes.[12] In addition to higher mean parameter values, the random parameters

[12]In studies based on models without random parameters, bias in population mean WTP arises from non-linearity in the calculation of WTP for any individual. With heterogeneity in the marginal utility of income, the average WTP will not equal the WTP of a person with average preference. Swallow *et al.*(91) and Souter and Bowker (95) showed average WTP to be higher than the WTP of a person with average preferences, and this result is consistent with the results

model yields estimated distributions of willingness to pay. All site and location attribute coefficient distributions have statistically significant standard deviations, indicating the population is in fact heterogeneous; a likelihood ratio test rejects the hypothesis that standard deviations are zero with $p < 10^{-103}$. The scale parameter is also significantly random, indicating the variance of unobserved utility varies among respondents.

In addition to being statistically significant, the magnitude of the attribute variances is economically significant. While the RPL agrees with the CLR model that groundwater quality is a high priority site attribute, the high mean of \$588.20 belies tremendous differences of opinion among residents, a standard deviation of \$363.28. This variance means that 94.8% of residents have positive values for groundwater, and 12.8% are willing to pay more than \$1000 a year in higher taxes to keep the landfill away from high quality groundwater supplies. In a state where many residents rely on groundwater for domestic use, such high values are not surprising. Those with negative values, who would prefer the landfill to be near high quality groundwater, could be developers hoping for the spread of municipal water systems or manufacturers or gas station owners who would like to be able to pollute groundwater. More likely, this mass is an artifact of the assumption of a symmetric distribution for population preferences. On this and other coefficients, the RPCLR model provides a sense of preference heterogeneity, and whether most people agree on the value of the attribute, or whether popular support around the attribute will be divided.

The fifth column of table 12 reports the mean marginal willingness to pay calculated from a RPL model with normally distributed attribute coefficients and

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of the present study.

a fixed cost coefficient;[13] the sixth column reports the standard deviations.[14] The results are qualitatively quite similar to RPCLR, as the (unreported) attribute coefficients from the RPL model indicate the means and standard deviations are significant and the willingness to pay values are approximately the same. However, the estimates have some undesirable statistical properties.[15] First, following common practice in simplifying the calculation of willingness to pay distributions, our RPL model estimated a fixed cost coefficient, thereby imposing a restriction that assumes away the possible heterogeneity in the marginal utility of income.[16] Thus, willingness to pay distributions are calculated by dividing estimated means and standard deviations of marginal utility by the constant marginal utility of income captured in the cost coefficient. However, this practice reintroduces some of the limitations RPL was designed to remove. Second, estimating a fixed cost coefficient only partially simplifies inference, as it leaves a significant computational task to determine the standard errors of moments of the welfare measure distributions.[17] In RPCLR, in contrast, those standard errors are reported directly in

[13]Since our purpose is to present an alternative approach to the widely used RPL with a fixed cost coefficient, we do not estimate a RPL with a random cost coefficient.

[14]WTP for the constant term implies that if the alternatives are similar in every aspect, then an individual is likely to choose the alternative B.

[15]The RPL results in table 12 would also be subject to nonlinearity bias (91; 95) due to constraining the marginal utility of income to a constant for the population. This source of non-linearity bias is avoided by RPCLR.

[16]This practice eliminates the possibility of estimating a, usually lognormal, cost coefficient distribution with a lot of mass near zero, resulting in distributions of willingness to pay with very fat tails, or worse, means and standard deviation that are not stable in the numerical division of the two distributions.

[17]Since, the objective of this paper is to show an efficient and computationally less intensive alternative to RPL for estimating welfare measures, rather than compare the WTP values from alternative models , the standard errors for the estimated WTPs are not calculated. However, the Krinsky and Robb (96) method can be used to compute standard errors by sampling from the joint distribution of the standard errors of the attribute coefficient mean, attribute standard deviation, the cost coefficient mean (and cost coefficient standard error) given by the estimated variance-covariance matrix. The samples can be used to generate a large number of willingness to pay distributions, using the population standard deviations of generated distributions' means and standard deviations as measures of statistical confidence. The distributions are also susceptible to having very fat tails, and hence very, very wide confidence intervals which may not be stable across replications of the Krinsky-Robb procedure.

the estimation. Finally, the assumption that distributions of marginal utilities are independent across attributes induces a strong correlation structure among willingness to pay (87), which makes it difficult to evaluate policies with different levels of attributes, since willingness to pay cannot be simply added across attributes.

3.3.2 Out of Sample Model Comparison

While there are several practical and statistical advantages to RPCLR over RPL, the models also differ in the measure on which distributional assumptions are being made, indicating one might fit better than the other. In the full sample, RPCLR has a significantly higher log-likelihood compared to the full sample RPL. To test out-of-sample predictive power, we randomly selected two of each respondents' questions and held them aside,[18] re-estimated the RPL and RPCLR models on the remaining data, and used the results to predict the held back responses for each person. To generate predictions we followed the procedure explained by Train ((3), chap. 11). First, we performed a Bayesian updating on the eight within-sample questions to calculate posterior distributions of β_i for each person, and then used these to calculate predicted choice probabilities for each out of sample question. The first two columns of table 13 show the percent correctly predicted,[19] aggregate squared error,[20] and log-likelihood of the out-of-sample predictions of the RPCLR and RPL models, respectively. RPCLR performs very similarly to RPL on all three measures. Thus, while we argue that the theoretical and practical advantages of RPCLR are sufficient to justify adopting our approach, within our application, there seems to be no tension between the practically and asymptotically superior

[18] For the few respondents with fewer than 7 responses, we held one random question aside; respondents with fewer than 4 responses were dropped from the sample.

[19] The percent correctly predicted is the percentage of observations in which the respondents chose the alternative the model predicts is more likely ($Pr(Y_i = j) > .5$). Although "percent correctly predicted" measure is often used to test the goodness of fit of an estimated model, Train warned against the use of this test statistics ((3), P. 73).

[20] Aggregate squared error: $\Sigma_N((\{I_i = 1\} - Pr(I_i = 1; x_i, \beta_i))^2 + (\{I_i = 0\} - Pr(I_i = 0; x_i, \beta_i))^2)$

model and the empirically superior model.

3.3.3 Correlated Attribute Distributions

Assuming distributions of willingness to pay are independent is no more sensible than assuming they have the correlation structure implied by assuming independent distributions of marginal utilities. However, the complexity of calculating estimated joint distributions of willingness to pay based on estimated joint distributions of marginal utilities is a barrier to capturing this element of variation in RPL. RPCLR can yield the joint distributions of attribute willingness to pay directly, as we demonstrate by estimating an RPCLR model with a full correlation structure allowed among main attribute effects. The seventh and eighth columns of table 12 report the means and standard deviations estimated through RPCLR with correlation. The estimated correlation matrix is shown in table 14.[21] Adding correlation does not dramatically change the conclusions drawn under RPCLR, though the absolute values of the estimates increase as more unobserved utility is associated with covariance of attribute values. Of the 36 estimated covariances, eight are significant at 5% or better, indicating that respondents' preferences for certain attributes are related.

Explicitly incorporating attribute preference correlation improves the fit of the model, as a likelihood ratio test rejects the independent correlation structure with a $p < 10^{-13}$ ($\chi^2 = 138.18$ with 36 degrees of freedom). However, the out of sample prediction rates, shown in the fourth column of table 13, indicate the lower log likelihood comes at a price of predictive power. The out-of-sample forecasting rate is actually 3% lower than RPCLR, with a higher aggregate squared error. Thus, even with more than 8000 in-sample observations, it seems including correlation

[21]The standard errors of the variance-covariance matrix are calculated by the derivative rule (75). The rule: $\psi = f(\rho), Var(\psi) = \frac{\delta\psi}{\delta\rho}' Var(\rho)\frac{\delta\psi}{\delta\rho}$; where ψ is the vector composed of the elements of the variance-covariance matrix of the random coefficients, Ω, and ρ is the vector composed of the elements of the lower triangular Choleski factor of Ω.

among nine attributes overfits the data.[22]

3.3.4 Site Evaluation

While the marginal willingness to pay estimates suggest which attributes are important to respondents, in practice policymakers must often select among different actual alternatives on behalf of their constituents. For the case study considered here, the relevant policy choice is selection of a landfill site. The objective is to identify the location that minimizes the welfare loss from the converted land and the attributes of its surrounding area. To facilitate application of our model, we follow Opaluch *et al.* (69) in calculating an index for each prospective site on the basis of the model estimates; the site with the highest absolute value of the index minimizes the loss. However, because CLR and RPCLR are money-denominated models, the index we calculate is the actual per-person welfare loss in value caused by developing the landfill. The monetary index for a site j can be defined as,

$$Index_{ij} = \beta_i x_{ij} - \text{cost}_{ij}. \tag{16}$$

We apply this monetary index to the two example sites originally evaluated in Opaluch *et al.* (69) by calculating a hypothetical referendum over the alternative sites described in (table 15). These monetary site indices can be transformed into estimated voting probabilities. The probability that person i votes for site A is given by,

$$P_{iA} = \frac{1}{1 + \exp[-(Index_{iA} - Index_{iB})/\sigma_i]}. \tag{17}$$

Since the βs are random so are the indices, and we approximate P_{iA} through simulation. We calculate P_A for R random draws of β_i from the estimated multivariate normal distribution, and then find the average of these probabilities as,

[22] A Monte Carlo analysis could provide guidence in choosing the model which has a better in-sample or out-of-sample fit, and which has better parameter recovery power, but does not help in finding which model is more numerically convenient or theoretically efficient, the attributes which are the basis of our argument.

$$P_{iA} = (1/R)\Sigma_1^R \frac{1}{1 + \exp[-(Index_{iA} - Index_{iB})/\sigma_i]}. \tag{18}$$

We calculated the indices following equation 16 and the voting probabilities following equation 18 for a representative Providence non-resident. These values for all the reported models are shown in table 16. In addition, we calculated expected vote shares from our heterogeneous sample population by updating the estimates in table 12 to reflect the observed individual choices ((3), chap. 11), obtaining posterior distributions for each β_i and aggregating across individuals. These expected vote shares are reported in the last row of table 16.

The expected average agent evaluation indices and voting probabilities, and the expected vote shares from our heterogeneous sample, all reflect a strong preference for site A in every model. This is consistent with our interpretation of the estimated coefficients. Respondents particularly avoid site B because of its high-quality groundwater, and gravitate toward site A because it is not near schools and can be accessed by highway.

In the money denominated models, the indices for each alternative increase about 10% in absolute value as preference variation assigned to the scale parameter in CLR is associated with variation in preferences for particular attributes in RPCLR, and again with covariances in preferences for particular attributes in RPCLR with correlation. As additional variation is captured in the heterogeneity structure, the models make less extreme predictions about voting probabilities for the average agent, with the majority for site A dropping from 92.8% to 88.8% between CLR and RPCLR, and to 76.3% for RPCLR with correlation. When individual voting probabilities are calculated by updating estimated population distributions based on an individual's choices, the predicted vote proportions become less extreme, losing between 12 and 16 percentage points, to fall to 77.0%

for RPCLR and 75.3% for RPL, and 60.1% for RPCLR with correlation. These lower vote shares suggest that the outcome of a referendum would not be nearly as clear cut as one would conclude in the absence of the more complex heterogeneous preference models.[23] Importantly for evaluating the difference between the RPL and RPCLR specifications, the two models yield remarkably similar vote shares by both calculation methods: 89% for the average agent and 75.3% and 77.0% expected vote shares across our heterogeneous sample. Since the complexity of these models' structure for capturing preference heterogeneity is comparable, we can again say that RPL and RPCLR yield similar results, but RPCLR yields them more directly.

3.4 Discussion

We have shown that by shifting the distributional assumption from error-scaled marginal utilities to money-scaled willingness to pay, it is possible to directly obtain estimates of distributions of willingness to pay for attributes of alternatives described in choice data. This approach has several practical advantages. It directly yields distributions, and the standard errors of their parameters, without additional calculation; it allows use of distributions such as lognormal for willingness to pay; the distributions of welfare measures are well behaved and do not have extremely fat tails, as can ratios of two distributions; and, in applications where correlation among distributions of WTP for different attributes can be rejected, dollar values can be directly summed to yield total willingness to pay for a policy

[23] These predictions are all more extreme than those of Opaluch *et al.* (69), who conclude site A captured a 55% majority vote. However, this result is based on their 194-variable demographic interaction model, and included a "nearby ponds" attribute present in only a few survey questions, which were not included in our dataset. If more moderate predictions are associated with a better model of preference variation, this suggests a properly specified demographic interaction model may be a better method for capturing heterogeneity than even a quite complex random parameters model. Our simple specification follows common practice and maximizes the heterogeneity for random parameters to capture, creating a good testbed for comparing the RPCLR and RPL models; comparing this to alternative specifications is beyond the scope of this paper.

alternative, rather than having to account for the correlation structure induced by a marginal utility-based independence assumption.

We argue that these advantages are sufficient to warrant adopting RPCLR for analysis of contingent choice data, rather than RPL, in the absence of compelling evidence that heterogeneity in willingness to pay is systematically better captured by the distributions induced by assuming standard distributions for marginal utilities. In the present application, RPCLR is not only easier to use, but it fits the data better than the RPL, in-sample. Train and Weeks (87) and Sonnier *et al.* (86) found that their models similar to RPCLR did not fit quite as well as RPL, but in neither case do we think the difference warrants the additional effort required to recover welfare measures, and their standard errors, from RPL.

The data set we used provides one important limitation to the generalization of our results and the strength of the empirical arguments supporting RPCLR. One of the key advantages to using random parameters models is that they can be specified to allow any substitution pattern among multiple choice alternatives, relaxing the independence from irrelevant alternatives assumption of standard conditional logit. The data set we chose for our illustration is a binary choice, on which the independence from irrelevant alternatives axiom is not binding. Hence, assuming distributions of willingness to pay may induce different substitution patterns among alternatives in choice problems with three or more alternatives. While it is difficult to envision any reason this should lead to a systematic difference with a marginal utility specification, whether there is a difference remains an empirical question.

Many discrete choice practitioners have moved toward random parameters models for measuring preferences in the hopes of greater statistical efficiency, and a better understanding of how policies will affect a heterogeneous population.[24]

[24]We leave the assessment of other methods, such as latent class or finite mixture logit models,

However, this appreciation of heterogeneity has been limited in the extent to which it affects policy analysis because of the complexity associated with deriving easily manipulable estimates of heterogeneous welfare measures. While Cameron and James' (88) and Cameron's (68) insight that welfare measures can be directly estimated provided minimal advantage in the fixed parameter context, it can be generalized to dramatically simplify analysis of distributions of welfare measures. With this approach for random parameters analysis, analysts can easily derive measures which will allow them to tailor policy recommendations to whole populations, rather than merely representative agents.

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outside the scope of this discussion.

Table 11. Description of variables included in the models

Variable types	Variable names	Variable descriptions.
Site attributes	Cost	Annual average cost per household.
	Farms	Difference between the acres of farm land in site A and site B.
	Marsh	Difference between the acres of marsh land in site A and site B.
	Groundwater	Difference in dummy variables indicating the presence or absence of high quality ground water in site A and site B.
	Wildlife	Difference in dummy variables indicating the presence of unique or normal wild life habitat in site A and site B.
Location attributes	Homes	Difference in the number of houses(100 homes) within 1 mile radius of location A and B.
	Park	Difference in dummy variables indicating the presence or absence of park land in location A and B.
	Farmland	Difference in dummy variables indicating the presence or absence of farm land in location A and B.
	School	Difference in dummy variables indicating the presence or absence of schools in location A and B.
	Highway	Difference in dummy variables indicating the presence or absence to highway access in location A and B.
Resident interactions	P×Farms	The interaction of P with the site attribute Farms.
	P×Marsh	The interaction of P with the site attribute Marsh.
	P×Groundwater	The interaction of P with the site attribute Groundwater.
	P×Wildlife	The interaction of P with the site attribute Wildlife.
	P×Homes	The interaction of P with the location attribute Homes.
	P×Park	The interaction of P with the location attribute Park.
	P×Farmland	The interaction of P with the location attribute Farmland.
	P×School	The interaction of P with the location attribute School.
	P×Highway	The interaction of P with the location attribute Highway variable.

Table 12. Full sample willingness to pay estimates from the alternative models

	CLR	RPCLR		RPL		RPCLR-correlated	
		Mean	Stds	Mean	Stds	Mean	Stds
Constant	-12.47	-15.01		*-17.32*		-4.24	
	(-1.409)	(-1.95)				(-0.58)	
Site Attributes							
Farms	-1.39	-1.62	2.39	*-1.68*	*2.50*	-1.71	2.44
	(-8.133)	(-8.40)	(8.68)			(-11.35)	(0.00)
Marsh	-0.554	-0.59	2.06	*-0.45*	*1.98*	-0.74	3.76
	(-3.167)	(-3.19)	(7.46)			(-4.14)	(0.00)
Groundwater	-556.89	-588.20	363.28	*-635.80*	*427.39*	-630.08	689.54
	(-16.956)	(-14.30)	(7.59)			(-16.53)	(8.14)
Wildlife	-248.98	-259.81	290.77	*-256.84*	*297.68*	-314.46	572.94
	(-10.086)	(-9.88)	(8.25)			(-11.66)	(8.14)
Location Attributes							
Homes	-30.35	-38.53	48.18	*-38.73*	*44.48*	-45.94	83.33
	(-14.346)	(-12.37)	(10.93)			(-12.88)	(0.31)
Park	-257.03	-262.71	113.72	*-266.93*	*131.70*	-309.83	325.36
	(-10.01)	(-10.42)	(3.42)			(-10.55)	(2.43)
Farmland	-324.91	-329.58	304.29	*-325.95*	*309.77*	-368.20	613.39
	(-12.128)	(-10.89)	(7.37)			(-12.12)	(14.04)
School	-367.97	-406.36	326.60	*-432.41*	*332.55*	-472.74	648.32
	(-13.362)	(-12.47)	(9.45)			(-13.54)	(25.76)
Highway	280.62	291.13	285.57	*287.39*	*-211.00*	302.21	512.32
	(11.01)	(10.02)	(7.87)			(11.16)	(18.21)
Providence Resident Interactions							
P×Farms	-0.38	-0.34		*-0.32*		-0.47	
	(-1.388)	(-1.13)				(-1.63)	
P×Marsh	0.75	0.64		*0.50*		0.75	
	(2.57)	(2.11)				(2.39)	
P×Groundwater	57.22	43.05		*45.14*		66.39	
	(1.41)	(0.90)				(1.64)	
P×Wildlife	49.63	28.78		*10.00*		46.18	
	(1.31)	(0.72)				(1.14)	
P×Homes	-1.99	-6.19		*-4.64*		-3.36	
	(-0.66)	(-1.40)				(-0.97)	
P×Park	64.71	50.95		*74.73*		88.65	
	(1.65)	(1.30)				(2.50)	
P×Farmland	81.97	53.92		*67.34*		103.03	
	(2.11)	(1.12)				(2.78)	
P×School	40.08	29.98		*78.09*		79.64	
	(1.04)	(0.64)				(2.05)	
P×Highway	-160.36	-196.16		*-178.00*		-181.05	
	(-4.08)	(-4.18)				(-4.90)	
Scale	396.68	5.46	0.76			5.18	1.89
	(22.93)	(85.11)	(9.36)			(56.21)	(-9.70)
Log-likelihood	-5954.079	-5699.947		*-5709.012*		-5630.848	

N=10703.The t-statistics are given in the parenthesis.

The fifth and sixth columns show the means and stds of the WTP distributions, calculated from the estimated RPL coefficient distributions. The price coefficient is -0.0044, and the mean and the standard deviation for the scale factor are 1.60 and 1.99 respectively.

For the RPCLR and the RPCLR with correlation, we have reported the mean and the standard deviation of $ln(scale)$. The mean and standard deviations of the log normally distributed scale is 313.81 and 277.46 for the RPCLR, and 1060.01 and 6234.48 for the RPCLR with correlation, respectively.

Table 13. Out of sample forecasting results

	RPCLR	RPL	RPCLR-correlated
Correct predicted choices	67.8%	67.7%	64.8%
Aggregate squared error	901.69	903.70	1046.85
Aggregate log-likelihood	-1316.04	-1317.77	-1475.09

Table 14. The correlation matrix of the WTP coefficients from the RPCLR-correlated model

	Farms	Marsh	Groundwater	Wildlife	Homes	Park	Farmland	School	Highway
Farms	1.00	-0.19	0.28	0.07	0.10	-0.10	0.22	0.11	-0.07
Marsh		1.00	0.53	0.21	0.03	-0.50	0.20	-0.04	-0.18
Groundwater			1.00	0.59*	-0.19	0.21	0.00	0.19*	-0.11
Wildlife				1.00	0.00	0.64*	0.16*	0.02	0.18
Homes					1.00	0.32	0.03	0.39	0.04
Park						1.00	0.61*	0.25	-0.07
Farmland							1.00	0.19*	-0.37*
School								1.00	-0.29*
Highway									1.00

The 5% significant covariances are indicated by asterisks.

Table 15. Attributes of the example sites

	Site A	Site B
Site Attributes:		
Farms	179.0 acres	52.0 acres
Marsh	50.9 acres	92.2 acres
Groundwater (1=high, 0=low)	0	1
Wildlife (1=unique, 0=normal)	0	0
Location Attributes:		
Homes (100 homes)	750.0 homes	600.0 homes
Park (1=presence, 0=absence)	1	1
Farmland (1=presence, 0=absence)	1	1
School (1=presence, 0=absence)	0	1
Highway (1=local roads, 0=highway)	1	0
Cost(household/year)	$83.60	$79.50

Table 16. Monetary indices of the hypothetical sites and voting percentages

Models	CLR	RPCLR	RPL	RPCLR-correlated
Mean index for Site A	-889.28	-991.78	-4.41	-1144.71
Mean index for Site B	-1891.33	-2034.96	-9.25	-2289.86
Representative agent's vote for site A	92.8%	88.8%	88.7%	76.3%
Expected vote for site A, based on the posterior distribution of β_i		77.0%	75.3%	60.1%

MANUSCRIPT 4

Conclusion and Recommendations

4.1 Summary

In this study, I examine how discrete choice experimental design can be improved to motivate individuals to truthfully respond to discrete choice questions and how discrete choice modeling can be modified to recover better welfare measurements from the observed data. From a design standpoint, I mainly focus on stated-choice experimental design. I develop a theoretical model of dominant strategy incentive compatible mechanism for designing stated-choice surveys in order to induce truth revelation among respondents. I present formal proofs of incentive compatibility for binary choice and multiple alternative choice cases.

The proposed mechanism is based on Clarke's (1) pivotal mechanism. For theoretical formulation, I impose some specifications on the utility functions. For both the binary and multiple alternative choice cases, utility functions are assumed to be quasi-linear. Although the quasi-linear utility assumption is restrictive, because it implies zero income elasticity thereby eliminating wealth effects, it is often used in applied welfare analysis. Often, researchers prefer a quasi-linear environment because it makes analysis extremely easy (e.g. Bagnoli and Lipman, (97)). I assume a quasi-linear utility environment because it is impossible to design dominant strategy mechanisms without this assumption. However, since income elasticity of demand for public goods can be non-zero, it is important to investigate the properties of this mechanism when the quasi-linear utility assumption is violated. As previously mentioned, in the absence of this assumption, dominant strategy will not exit. In their discussion about the limitations of the demand revealing mechanism, Groves and Ledyard (98) mentioned that the presence of income effect in this mechanism might lead to instability and strategic manipulation. But, they further commented (99), given that there does not exist an ideal collective decision making mechanism, whether these are more serious concerns for the demand revelation mechanism than other mechanisms, is an empirical question. Since the DCPCM considers the same quasi-linear utility environment as the demand revealing mechanism, Groves and Ledyard's comments regarding income effect also apply here.

Further, for the multiple alternative choice case, to rule out substitution and complementary relations between alternatives, the quasi-linear utility function is assumed to be strongly separable. Allowing for these effects violates the dominant strategy property. Consider a case where an individual prefers either of the alternatives but not necessarily both. Now suppose he reveals his true preference by choosing his most preferred alternative, but given the others' preferences, his most preferred alternative can not be implemented. However, had he chosen his next preferred alternative, then it would have been possible to provide that alternative, and he would have been better off. This implies that his truth does not guarantee him the best outcome. It is possible to present several similar scenarios where such outcome is possible. This situation does not arise in an open-ended question format if individuals reveal their values for each alternative and all their possible combinations, but it is likely to emerge in a discrete choice framework where individuals choose only one alternative from a set of alternatives. The separable utility assumption, although restrictive, is somewhat realistic given the conflicting evidence about the presence of substitution and complementary effects in public good valuation. For example, Hoehn and Loomis (100) found evidence of significant substitution effect in their multiple environment program valuation study. In a similar study, Hailu *et al.*(66) found a complementary effect instead of substitution effect between alternatives. This implies, in practice, whether this mechanism will be successful in truth revelation is an empirical question.

Another limitation of this proposed mechanism is that although it achieves efficient project choice, similar to the pivotal mechanism, the outcome is rarely fully Pareto efficient. An allocation is Pareto efficient if it is impossible to make one individual better off without making someone else worse off. Pareto efficiency also requires that there is no wastage of numeraire; that is the mechanism satisfies budget balance condition

$$\sum_i t_i - T = 0, \tag{19}$$

where t_i is the tax to individual i and T is the project cost. If the outcome is such that the budget is balanced, then the outcome is fully efficient. On the other hand, outcome is not efficient if $\sum_i t_i - T > 0$; that is, surplus exists. This surplus cannot be allocated among the respondents because that would distort the incentives of the individuals and, therefore violate the dominant strategy property. The outcome is also not efficient if there is a deficit; that is, $\sum_i t_i - T < 0$, in which case funding has to come from elsewhere in the economy. This efficiency cost is not considered to be a cause of serious concern because of the impossibility of achieving a social choice that is dominant and also Pareto efficient.

I also design induced-value experiments to test the predictions from the theoretical model, compare the performance of the proposed mechanism against alternative mechanisms, and analyze how individuals' incentives are influenced by the proposed mechanism. I use non-parametric and econometric techniques to analyze the experimental data. The experimental results of the binary choice show that the proposed model performs very well with 83% true response rate. The mean willingness to pay estimate from the econometric analysis is \$12.63 which is also close to the mean induced value of \$12.50. Interestingly, these results are very close to the results obtained from the DC-PPMBG. On the other hand, contrasting results are obtained for the multiple alternative choice case, where the true response rate in the DC-PPMBG treatment (78.22%) is higher than the same in the DCPCM (72%). I also find that, unlike the binary choice case, there is a clear learning trend among individuals in the multiple choice DCPCM, but experience gained from participating in the DC-PPMBG does not improve truth revelation. Since how accurately an experiment predicts the performance of a theory depends on its design, special attention has been given to the experimental design through proper group assignment, treatment ordering and administrative aspects such as clearly written instructions, pilot study, quiz and practice rounds.

Although experimental methods have emerged as powerful tools for evaluating economic propositions over the last two decades, skepticism exists surrounding their use. The most common argument against experimental methods is that the real world is much more complicated; whereas laboratory environments are fairly simple. Despite simplicity, one must not forget that laboratory markets are real markets in the sense that real people follow real rules and make real decisions in the pursuit of real profits. General theories and models designed for complex, real markets are expected to work in all special cases, including laboratory markets. There are examples where experiment data helped analysts shape their hypotheses and beliefs about the more complex, real-world environment. For example, laboratory applications found that the PPMBG increases contribution (32; 27; 24), and similar results were obtained from field applications of the PPMBG (23; 14). However, if a theory performs in the simple laboratory environment, it does not necessarily mean that these results will carry over to the real environment, but if a theory fails in a laboratory environment, then there is hardly any hope that it will perform in the natural environment. That is, laboratory markets are helpful in validating or rejecting a theory or model in light of experience gained from them.[1]

[1]More detail about the relevance of experimental methods in evaluating economic theories

Another reservation concerns the use of students in economic experiments. This criticism is also more of a criticism about the choice of subjects rather than about the usefulness of experimental methods. Several studies have been undertaken to compare the behavior of students with that of experienced decision makers (102; 103; 104), but no substantial difference in behavior was reported. For example, Dyer *et al.* (102) observed similar behavior from both students and business executives in their sealed-bid common value offer auction study. The choice of a particular subject pool might be an issue if typical economic agents think very differently from students. In this particular case, the real economic agents are the general public,, who may or may not have any experience in public good choice. Since students are part of this society and are subject to decision making as much as the general public, I do not expect subject experience to be a cause of concern.

The other part of this study improves on discrete choice modeling by proposing an alternative way to estimate willingness to pay distributions for heterogeneous populations. Recently, several discrete choice models have been developed in order to capture the complex heterogeneous structure in individual preference functions; namely, random parameter logit models and mixture models. Among these models, random parameter logit became quite popular, since it assumes each individual to have his own preference function.

Despite the popularity of random parameter logit, the constant cost assumption of most random parameter applications is a serious limitation. This study suggests an alternative model specification that allows estimation of an expenditure function instead of a utility function, where the estimated coefficients can be directly interpreted as willingness to pay distributions. An empirical application reveals that the results are qualitatively similar in both the proposed model and in the random parameter logit model. Although both models perform very similarly

can be found in Plott (101; 51).

in and out of sample, the proposed model yields the willingness to pay estimates along with their significance measures more directly, easily and with fewer assumptions. The study also demonstrates how correlations among preferences can be easily built into the proposed model.

4.2 Contribution to Literature

Implications for Stated Choice Design

Although recently the use of stated choice methods became widespread for eliciting individuals' preferences, strategic bias remains to be a continuous source of debate among researchers. Several econometric and theoretical methods have been employed to deal with this issue. Recent development in this area includes designing discrete choice questions with provision point money back guarantee. Although PPMBG removes these biases significantly in practice, incentives to misrepresent preferences still remain. This study contributes by drawing on incentive compatibility literature to remove these incentives. This mechanism can correctly elicit individuals' preferences and therefore help managers and policy makers in making optimal decisions consistent with society's interest. In addition, it can provide an accurate reference point to test for the existence of hypothetical bias. I expect that this research will help settle some debates surrounding the accuracy of stated choice results.

This study also contributes to the mechanism design literature. Researchers have developed a variety of sophisticated incentive-compatible mechanisms that discuss how socially optimal decision can be reached based on individuals' self-interested behavior. Most of these previous mechanisms are theoretically appealing, but they have two major limitations and consequently had limited or no success in solving the real social allocation dilemma. First, they have very complicated incentive structures, making them almost impossible to implement. Second, most of these incentive-compatible mechanisms are largely based on open-ended response formats. As discovered by previous studies (7; 105; 106), when compared to discrete choice format, open-ended format leads to undervaluation of public goods. Early problems that researchers found with open-ended question were that agents were confused about the cost information. They did not understand why they were not provided with cost information if the decision-making agency had worked out the details regarding how the good would be provided. Several researchers also noted the obvious advantage of discrete choice questions. Discrete choice questions leave respondents in a familiar position because they only react to a posted price, as they do while shopping at actual markets. It is probably easier for the respondents to make a choice among a set of alternatives at given costs than formulating a continuous WTP response as required in open-ended questions.

The mechanism that I propose here not only has a relatively easy incentive structure and is easy to understand, but it is also easy to implement. The experimental results support these hypotheses. Moreover, discrete choice question framework avoids the complications associated with the value formation for individuals. As expected, this study finds that the value revealed under the stated choice framework is double the value revealed from the open-ended framework.

Implications for Discrete Choice Modeling

This study contributes to the discrete choice modeling research that suggests improved ways to recover individual WTP measures from observed data, including both revealed preference and stated preference data. A significant advance has been made in the discrete choice modeling front, such as the formulation of random effects models, mixture models, and random parameter logit models. Although some of these models have existed for long time, the lack of proper computer power prohibited their estimations. Recently, the tremendous growth in computer hardware and software has made estimations of these complicated models possible. Now, it is also possible to handle numerical integration, including several random variables with complicated distributions. All the advancements on this front consisted of inventing several ways to take account of the heterogeneous structures of individuals. Not much attention has been given to tackle the constant cost coefficient assumption, which is a common criticism often raised against random parameters logit modeling. Typically, practitioners choose to make this assumption to avoid the complications arising from a random cost coefficient. However, the constant cost coefficient assumption is not an accurate assumption, because studies that allowed the cost coefficient to vary, found it to be random. For example, Train (78) and Hall *et al.* (84) estimated random parameter logit models with random cost coefficient and found them to vary significantly among respondents. This paper discusses how Cameron's (68) censored logistic regression criterion, adjusted for heterogeneous populations, allows the estimation of distribution of willingness to pay without any numerical complications that arise following a random cost coefficient.

Although Cameron's (68) idea has existed since 1987, its use did not really become widely implemented because it provided no advantage over the traditional logit or probit models. But for heterogeneous populations, Cameron's idea of directly estimating welfare measurements has some clear advantages. Some studies have been pursued on direct willingness to pay estimation by Train and Weeks (87) and Sonnier *et al.* (86), but all these studies adopted a Bayesian estimation method. This paper first discusses the use of classical estimation method. Although the Bayesian estimation method is becoming extremely popular among researchers, it also has its disadvantages (107). First, formulating a prior can be a daunting task for non-Bayesian practitioners. Second, the learning curve is quite steep given that the algebra for Bayesian analysis is very complex and mathematically demanding. The classical estimation approach is relevant, because it would help researchers who are well versed in classical methods and unwilling or not ready to switch to the Bayesian method, adopt this new approach with ease. Again, since our exposition follows directly from Cameron's formulation, it would also help environmental practitioners who are already familiar with Cameron's approach.

4.3 Recommendations for Future Research

There are several ways this research can be extended. The extension can be conducted from both the theoretical and experimental fronts. More insights about the performance of the mechanism and about individuals' incentives can be gained by changing the experimental design. In this present study, I found existence of both under-revelation and over-revelation of preferences among the non-dominant strategy plays. I argue that deviations from equilibrium strategies might be the result of altruism, warm glow, or confusion. This experimental design can be modified to test for these effects.

There are several studies which investigate the influence of altruism, warm glow and decision errors on the experimental decision making process (56; 55; 57). For example, Palfrey and Prisbrey (55) studied the anomalous behavior of subjects in voluntary contribution experiments. They found almost little or no effect of altruism on individual decisions but found significant effects of warm glow and random errors. Goeree *et al.* (57) not only found evidence of altruism but also observed heterogeneity in the degree of altruism among individuals. Other examples include, Videras and Owen (108), Menges *et al.* (109), Croson (110) and Ma *et al.*(111). It would be interesting to pursue more research to investigate if the demand over-revelation in fact result from warm glow, altruism or confusion.

Again, one major concern about the pivotal mechanism is that the incentive for truthful revelation might dissipate when group size is very large. When the group is very large, an individual might realize that the probability of he being pivotal is very small. Therefore, the cost of his decision might not be tangible to him anymore, leading him to contribute against his dominant strategy. This raises the question of whether the DCPCM would give rise to a bias similar to hypothetical bias in a large group. This hypothesis can be tested by studying the effect of group size on truth revelation.

For the multiple alternative choice case, I have assumed strongly separable utility functions in order to eliminate substitution and the complementary relationship among the project alternatives. However, in reality, people often have preferences inclusive of substitution and complementary effects. It would be more realistic to extend this model to incorporate these effects. As I have discovered, allowing for substitution and complementary effects leads to a failure of the dominant strategy equilibrium. Instead, there will be multiple Nash equilibria. I do not expect the presence of substitution and complementary effects to have a significant impact on truth revelation. In that case, although theoretically it is not the best strategy for an individual to reveal his true preference, in practice he might still respond truthfully, because given several alternatives and their combinations and without any information about others' preferences, it might be extremely difficult for him to devise a strategic response that would serve him the best. It would be interesting to explore the nature of the equilibrium in the absence of separable utility functions. The finding would then make this approach more general and practical.

Following the multiple choice results, I conjectured that the lower truth revelation rate might also be a result of the complicated pivotal tax definitions. This effect can be tested by designing an experiment where subjects participate first in the binary choice treatment and then in the multiple alternative choice treatment.

Extension of the research idea expressed in manuscript III is also possible. The limitation of this study is that I have estimated linear utility and WTP functions, thereby eliminating wealth effects. This is not so much a limitation for a public good case, but might not be appropriate for a private good case. Since the application of this model is not limited to public goods, it is essential to conduct analysis that allows for income effect. It would make this approach appealing to other disciplines that use discrete choice data, such as market research. It would also be beneficial to test the robustness of this method with different data sets; data sets with multiple choice alternatives; and other types of parameter distributions, such lognormal and triangular. I believe more research in this area might finally channel the heterogeneous discrete choice modeling research into this new direction, from models in utility space to models in willingness to pay space.

In conclusion, this research adds to the body of knowledge on incentive compatible mechanism design and stated choice survey methodology. The findings of this research are encouraging, but more research is needed before it can be successfully implemented in the field for valuing real public goods. These incentive compatible survey responses, when combined with the discrete choice modeling approach explained here, will enrich the welfare measurements and hence help policy makers and managers make more informed, efficient decisions. From the modeling perspective, the conventional practice is to estimate utility coefficients and then transform them to obtain welfare measures. However, it is more sensible to estimate willingness to pay directly when the ultimate objective of the analyst is to estimate willingness to pay rather than the utility coefficients. There is an additional statistical and analytical advantage of estimating welfare measures directly when the population is heterogeneous. Given evidence of the population being heterogeneous and the limitations of present heterogeneous modeling, this alternative method needs serious consideration.

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APPENDIX A

Instructions for The Binary DCPCM Experiment

A.1 Introduction

This is an experiment in group decision making. During the experiment, you will be earning money in "experimental dollars." Your show-up fee has been converted to experimental dollars for you to use during the experiment. At the end of the experiment, your fee and additional earnings will be converted to real dollars and you will be paid in real dollars as you leave.

Today you will participate in three different experimental treatments. We will now instruct you on the basic game and treatment I. Following treatment I, there will be another set of instructions that will precede treatment II, and a final set of instructions prior to treatment III.

The basic game

In this experiment, you will make decisions in a group of five subjects. Together with the four other members of your group, you will decide whether to implement or not to implement a project that provides a benefit to each group member. You must decide how much of your money you will contribute toward the cost of the project.

How you make money

All members of the group receive a benefit when the project is implemented. The project is implemented when the total contribution by all the members of your group equals or exceeds the cost of the project. If the project is implemented, you will receive your value for the project. If the project is not provided, you will receive nothing.

The levels of contributions you may choose, and the way the payment is calculated based on your offered contribution, will change between treatments. Your profit will be your value minus your payment. Your task is to choose levels of contributions so that you make as much profit as possible.

Values

At the beginning of each period, each subject will learn his or her private value for that period's project. Values are randomly chosen between 5 and 20, with all values equally likely. Different subjects have different values for each period.

Periods

You will play the basic game several times during the experiment, once in each period. At the beginning of each period, all subjects will be randomly assigned to a new group. Treatment I will have thirty periods, followed by fifteen periods of both treatment II and treatment III.[1] Before each treatment, you will do a practice round and a quiz, which will help you understand the rules of the games. The quiz is designed to demonstrate how your decision affects your payment and your profit given the decisions of your group members.

Your final income

You will receive 20 experimental dollars as an initial fund, which will reset at the beginning of each treatment. This initial fund will be displayed as your starting total profit. Your profit from each period will be added to this total profit and your loss will be subtracted from this amount.

A.2 Treatment I

In treatment 1, you will learn your value and then be asked to select any amount as your contribution toward the project. If the total contribution of all

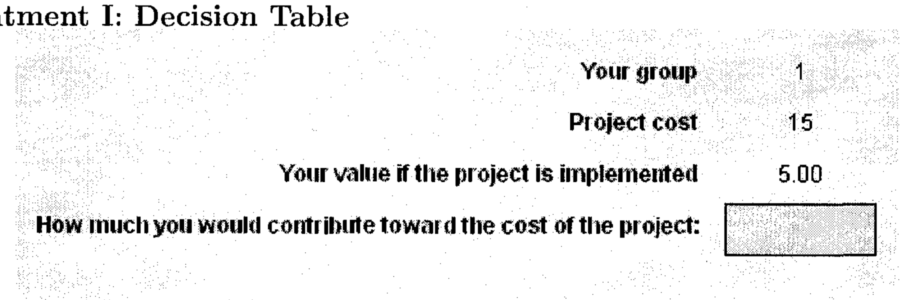
[1]This statement was revised before each session to reflect the different ordering of the treatments among sessions.

group members equals or exceeds the project cost, the project will be implemented and you will receive your value. Your profit is your value minus your payment. If the total contribution of all group members is less than the project cost, the project is not implemented. Your contribution will be returned to you, and your payment and profit will be zero for that period.

Decision software

The figure below shows an example of the decision table that you will use to make your contribution decision. The table indicates that in the current period, you are assigned to group number 1, that group 1's project costs 15 experimental dollars to implement, and that your value if the project is implemented is 5 experimental dollars. That is, you will receive 5 experimental dollars if the total contribution from all members of your group is at least 15 experimental dollars. Once you make a decision about how much to contribute, you may type the amount in the box below and click the OK button.

Treatment I: Decision Table



Your group	1
Project cost	15
Your value if the project is implemented	5.00
How much you would contribute toward the cost of the project:	

After all group members have submitted their decisions, your screen will display the results. In the example result table below, the project is not implemented. Despite your contribution of \$4, the cost of \$15 was not met.[2] As a result, you paid nothing, and received no benefit from the project, giving you zero profit this period.

[2] All dollars are experimental dollars.

Treatment I: Result Table

Project cost	15
Your contribution	4.00
Group decision (Yes= project implemented, No=project not implemented)	No
your benefit received	0.00
Your payment	0.00
Your profit	0.00

Summary

1) At the beginning of each period, all subjects are randomly reassigned to groups of five.

2) You learn your private value for the project, with values between 5 and 20 equally likely. Different subjects receive different values.

3) You choose a contribution toward the project.

4) The project is implemented if the total contribution of all group members equals or exceeds the project cost.

5) If the project is implemented, you receive your value; you receive nothing if the project is not implemented.

6) Your payment is equal to your contribution; your profit is your value minus your payment.

Questions

Since your earnings depend on the decisions you make, it is very important that you understand the procedures and how your earnings will be calculated. If you have any questions, please raise your hand now and the experimenter will answer them.

Practice period

We will now demonstrate this treatment through a practice period. Your earning from the practice period will not count toward your earnings for the experiment. The project cost is 15 experimental dollars for this practice period.

Practice period 1

1) For practice period 1, everyone will have the same value for the project, $5.

2) For this practice period, everyone will choose a contribution of $4. Click on the contribution box and enter 4.

3) Click the OK button to view the results.

4) Since everyone in your group contributes $4, the total contribution of your group is, 5 x 4 =20, which is greater than the project cost $15.

5) Therefore the project is provided; and your payment is 4 and profit is (your value -your payment)= (5 - 4)=1.

6) Click OK to exit the practice screen.

After the practice period, you will be given a quiz, which will help you understand the rules of the game. After the quiz, the real experiment will begin. Your values and costs will be different in each period and also different from those of the other subjects. Once the experiment begins, there will be no communication among subjects, apart from the transmission of responses by the computers.

A.2.1 Quiz

Your group consists of five members. The project cost is 15 experimental dollars. Please answer the following questions based on the rules of treatment I.

Question 1

Your value is 5 experimental dollars and your contribution is 3 experimental dollars. The total of the contributions by the other four members of your group is 10 experimental dollars. Fill in the section below for this situation.

1) The total contribution of the group: ________________

2) Project will be implemented: YES [ ], NO [ ]

3) Your profit (your value - your contribution):________________

Question 2

Your value is 6 experimental dollars and your contribution is 4 experimental dollars. The total of the contributions by the other four members of your group is 12 experimental dollars. Fill in the section below for this situation.

1) The total contribution of the group: ________________

2) Project will be implemented: YES [ ], NO [ ]

3) Your profit (your value - your contribution):________________

A.3 Treatment II

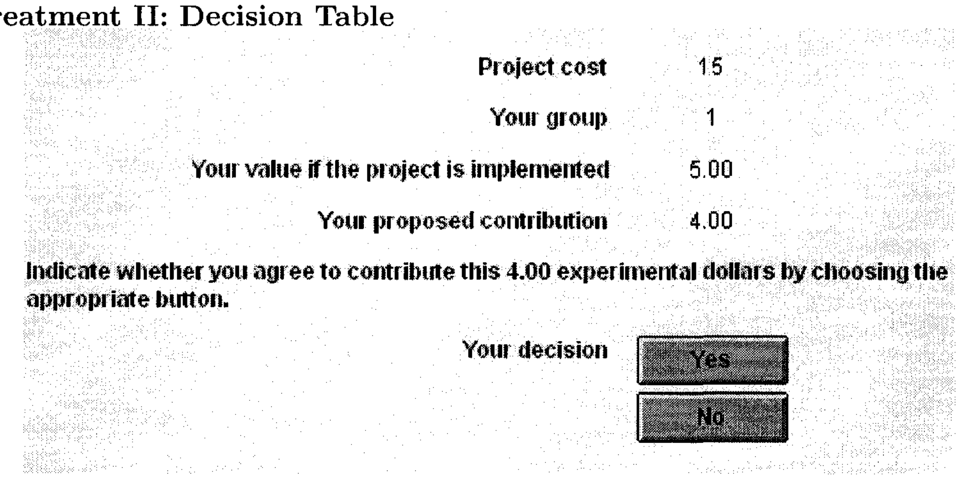
As in the previous treatment, at the beginning of each period, you will be randomly assigned to a new group of five subjects and learn your private value for the project. However, you will also be given a proposed contribution, which is randomly chosen between 2 and 30. You must decide whether to accept this proposed contribution and say *Yes*, or to reject this proposed contribution, and say *No*. If you say *No*, your contribution is taken to be zero.

If the total contribution of all group members equals or exceeds the project cost, the project will be implemented and your payment will be equal to your contribution. Your profit for that period will be your value minus your payment. If the total contribution of all group members is less than the project cost, the project is not implemented. Your contribution will be returned to you, and your payment and profit will be zero for that period.

Decision software

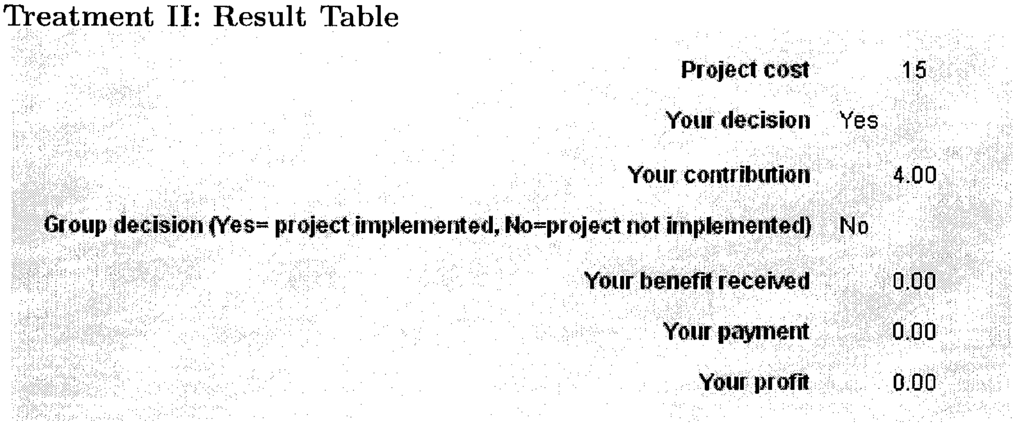
The figure next page shows an example of the decision table that you will use to make your decision in treatment II. The table indicates that in the current period you are assigned to group number 1 and that group 1's project costs 15 experimental dollars to implement. Your value if the project is implemented is 5 experimental dollars and your pre-assigned proposed contribution is 4 experimental dollars. That is, if the total contribution from all members of your group is at least 15 experimental dollars, you will receive 5 experimental dollars. You will pay 4 experimental dollars if your decision was *Yes* and the project is implemented. You must decide whether or not you want to contribute 4 experimental dollars toward this 15 experimental dollars project cost.

Treatment II: Decision Table



After all group members have submitted their decisions, your screen will display the results. In the case shown here, you indicated you would make the proposed contribution of 4 experimental dollars. However, the total contribution to which other members of your group said *Yes* were less than \$11, so total contribution was less than the project cost of \$15. As a result, the project is not implemented, you paid nothing, and received no benefit from the project, giving you zero profit this period.

Treatment II: Result Table



Summary

1) At the beginning of each period, all subjects are randomly reassigned to groups

of five.

2) You learn your private value for the project, and a private proposed contribution for the project. Different subjects receive different values and proposed contributions.

3) You decide whether to say *Yes* and accept this proposed contribution, or say *No* and reject this proposed amount. Your contribution is taken to be zero if your response is *No*.

4) The project is implemented if the total contribution of all group members equals or exceeds the project cost.

5) Your payment is equal to your contribution if the project is implemented, otherwise it is zero.

6) If the project is implemented, your profit is your value minus your payment. If the project is not implemented, you receive or pay nothing and your profit is zero.

Questions

If you have any questions, please raise your hand now and the experimenter will answer them.

Practice period

We will now demonstrate this treatment through a practice period. Your earning from the practice period will not count toward your earnings for the experiment. The project cost is 15 experimental dollars for this practice period.

The following steps demonstrate how your and your group members' decisions are used to determine the project implementation decision, your payment and your profit. All of the following information for each subject is shown in Table A.1.

Practice period 1

1) You learn your value and proposed contribution for the project. All subjects

have a value of \$6, and some have proposed contributions of \$5 and others of \$7. Values and proposed contributions for all five subjects are shown in Rows A and B of Table A.1.

2) For this practice period, subjects with proposed contributions of \$5 will contribute that amount, and those with proposed contributions \$7 will contribute zero. Click, *Yes*, if your proposed contribution is \$5, click *No* if it is \$7 (Row C).

3) Click the OK button to view the results of your decision.

4) After everyone has entered his or her decision, the group decision is determined. Since, total contribution of the group is, 0+0+ 5+5+5=15, the project is implemented (Row E).

5) Since the project is implemented, you pay the amount you agreed to contribute (Row F) and your profit (Row G) is your value minus your payment (Row A-Row F).

6) Click OK to exit the practice screen.

Table A.1. Practice period 1

Your Number	1	2	3	4	5
A) Your value	6	6	6	6	6
B) Your proposed contribution	7	7	5	5	5
C) Your decision	No	No	Yes	Yes	Yes
D) Your contribution	0	0	5	5	5
E) Group decision			Yes		
F) Your payment	0	0	5	5	5
G) Your profit	6	6	1	1	1

After the practice period, you will be given a quiz, which will help you understand the rules of this game. After the quiz, the real experiment will begin. Your values and costs will be different in each period and also different from those of the other subjects. Once the experiment begins, there will be no communication among subjects, apart from the transmission of responses by the computers.

A.3.1 Quiz

Your group consists of five members. The project cost is 15 experimental dollars. The following questions present four different situations. Your value and your proposed contribution under these situations are displayed in four different tables. Column 5 of each table displays the sum of the amounts to which the other four members of your group said *Yes*, and thus agreed to contribute. Suppose your decisions under each situation are as shown in column 4 of each table. Now answer the following questions based on the information given in each table, and according to the rules of treatment II. Please note that during the experiment, you will not know the other group members' values or contributions.

Question 1

Situation	Your value	Proposed contribution	Your decision	Other members' contributions
1	6	7	No	15

Fill in the section below for situation 1.

1) The amount you agreed to contribute: ____________________

2) The total contribution of the group: ____________________

3) Project will be implemented: YES [ ] NO [ ]

4) Your payment :____________________

5) Your profit (value-your payment): ____________________

Question 2

Situation	Your value	Proposed contribution	Your decision	Other members' contributions
2	6	7	Yes	15

Fill in the section below for situation 2.

1) The amount you agreed to contribute: ____________________

2) The total contribution of the group: ____________________

3) Project will be implemented: YES [ ] NO [ ]

4) Your payment :______________________________

5) Your profit (value-your payment): ____________________

Question 3

Situation	Your value	Proposed contribution	Your decision	Other members' contributions
3	6	7	Yes	7

Fill in the section below for situation 3.

1) The amount you agreed to contribute: ____________________

2) The total contribution of the group: ____________________

3) Project will be implemented: YES [ ] NO [ ]

4) Your payment :______________________________

5) Your profit (value-your payment): ____________________

Question 4

Situation	Your value	Proposed contribution	Your decision	Other members' contributions
4	6	5	Yes	10

Fill in the section below for situation 4.

1) The amount you agreed to contribute: ____________________

2) The total contribution of the group: ____________________

3) Project will be implemented: YES [ ] NO [ ]

4) Your payment :______________________________

5) Your profit (value-your payment): ____________________

A.4 Treatment III

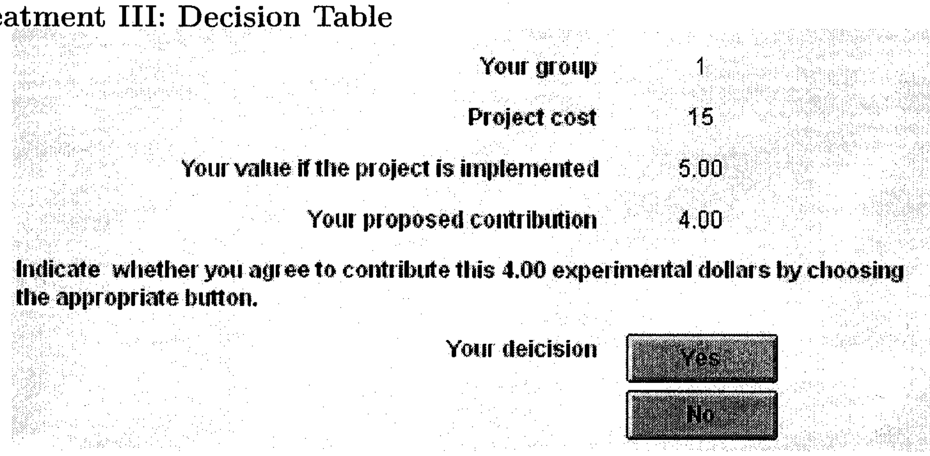
As in the previous treatment, at the beginning of each period, you will be randomly assigned to a new group of five subjects and learn your private value for the project. However, you will also be given a proposed contribution, which is randomly chosen between 2 and 30. You must decide whether to accept this proposed contribution and say *Yes* or to reject this proposed contribution, and say *No*. If you say *No*, your contribution is taken to be zero.

If the total contribution of all group members equals or exceeds the project cost, then the project will be implemented. However, in this treatment what you pay depends not only on your decision and the group decision but also on the total contribution by the other four members of your group. If the total contribution of the other four members is sufficient to provide the project without your contribution, then you pay nothing in spite of your acceptance of the proposed contribution. In this case, your profit is your value. However, if your contribution is required to meet the project cost given the others' contributions, then you will pay the amount you agreed to contribute. Here your profit is your value minus your payment. Therefore, you pay only if your contribution makes the difference between providing the project and not providing the project. If the total contribution of all group members is less than the project cost, the project is not implemented. Your contribution will be returned to you, and your payment and profit will be zero for that period.

Decision software

The figure next page shows an example of the decision table that you will use to make your decisions in treatment III. The table indicates that in the current period, you are assigned to group number 1 and that group 1's project costs 15 experimental dollars to implement. Your value if the project is implemented is 5 experimental dollars and your proposed contribution is $4. That is, if the total contribution from all members of your group is at least $15, you will receive 5 experimental dollars. You will pay $4 if your decision was *Yes* and the total amount, the other members of your group agreed to pay is between $11 and $15, where $11 is the project cost minus your contribution. If the total contribution by other members is at least $15, then the project will be implemented and you will not be required to pay anything in spite of your acceptance of the proposed contribution. If the total contribution by other members is less than $11, then the project cannot be implemented despite your contribution. In this case, your contribution will be returned and the project will not implemented. You must decide whether or not you want to contribute $4 toward this $15 project cost.

Treatment III: Decision Table



After all group members have submitted their decisions, your screen will display the results. In the case shown here, you indicated you would make the proposed contribution of $4 if it made the difference between providing the project and not providing the project. However, the total proposed contribution to which other members of your group said *Yes* was less than $11, so total contribution of your group is less than the project cost of $15. As a result, the project was not implemented, you paid nothing and received no benefit from the project, giving you zero profit this period.

Treatment III: Result Table

Project cost	15
Your decision	Yes
Your contribution	4.00
Group decision (Yes=project implemented, No=project not implemented)	No
Decision by other members of your group	No
Your benefit received	0.00
Your payment	0.00
Your profit	0.00

Summary

1) At the beginning of each period all subjects are randomly reassigned to groups of five.

2)You learn your private value for the project and a private proposed contribution for the project. Different subjects receive different values and proposed contributions.

3)You decide whether to say, *Yes* and accept this proposed contribution, or say *No* and reject this proposed amount. Your contribution is taken to be zero if your response is *No*.

4)The project is implemented if the total contribution of the group equals or exceeds the project cost.

5) You pay the amount you agreed to contribute if your contribution is required to meet the project cost given the others' contribution, and pay nothing in all other cases.

6) If the project is implemented, your profit is your value minus your payment. If the project is not implemented, you receive or pay nothing and your profit is zero.

Questions

If you have any questions, please raise your hand now and the experimenter will answer them.

Practice period

Now, we will demonstrate this treatment through a practice period. Your earning from the practice period will not count toward your earnings for the experiment. The project cost is 15 experimental dollars for this practice period. The following steps demonstrate how your and your group members' decisions are used to determine the project implementation decision, your payment and your profit. All of the following information for each subject is shown in Table A.2.

Practice period 1

1) You will learn your value and a proposed contribution for the project. All subjects have a value of $6, and some have proposed contributions of $5 and others of $7. Values and proposed contributions for all five subjects are shown in Rows A and B of Table 1.

2) For this practice period, subjects with proposed contributions of 5 will contribute that amount, and those with proposed contributions of $7 will contribute zero. Click, *Yes* if your proposed contribution is $5, click *No* if it is $7 (Row C).

3) Click the OK button to view the results of your decision.

4) After everyone has entered his or her decision, the group decision is determined. Since, total contribution of the group is, 0+0+5+5+5=15, equals to the project cost, the project will be implemented (Row E).

5) If the total contribution of the other four members of your group (Row F) is greater or equal to the project cost, then the project is implemented without your contribution (Row G) (subjects 1 and 2 pay nothing).

6) Your contribution is required to meet the project cost if the total contribution

Table A.2. Practice period 1

Your Number	1	2	3	4	5
A) Your value	6	6	6	6	6
B) Your proposed contribution	7	7	5	5	5
C) Your decision	No	No	Yes	Yes	Yes
D) Your contribution	0	0	5	5	5
E) Group decision	Yes				
F) Sum of other members' contributions	15	15	10	10	10
G) Project decision by other members	Yes	Yes	No	No	No
H) Your payment	0	0	5	5	5
I) Your profit	6	6	1	1	1

of other four members of your group is between \$10 and \$15. Then you pay the amount you agreed to contribute, and your profit is your value minus your payment. This is true for subjects 3, 4 and 5.

7) Click OK to exit the practice screen.

After the practice period, you will be given a quiz, which will help you understand the rules of the game. After the quiz, the real experiment will begin. Your values and costs will be different in each period and also different from those of the other subjects. Once the experiment begins, there will be no communication among subjects, apart from the transmission of responses by the computers.

A.4.1 Quiz

Your group consists of five members. The project cost is 15 experimental dollars. The following questions present four different situations. Your value and your proposed contribution under these situations are displayed in four different tables. Column 5 of each table displays the sum of the amounts to which the other four member of your group said *Yes*, and thus agreed to contribute. Suppose your decisions under each situation are as shown in column 4 of each table. Now answer the following questions based on the information given in each table, and according to the rules of treatment III. Please note that during the experiment, you will not know your other group members' values or contributions.

Question 1

Situation	Your value	Proposed contribution	Your decision	Total of other members' contributions
1	6	7	No	15

Fill in the section below for situation 1.

1) The amount you agreed to contribute: ____________

2) The total contribution of the group: ____________

3) Project will be implemented: YES [ ] NO [ ]

4) Can the project be provided without your contribution: YES[ ] NO [ ]

5) Your payment: ____________

6) Your profit (value-your payment): ____________

Question 2

Situation	Your value	Proposed contribution	Your decision	Total of other members' contributions
2	6	7	Yes	15

Fill in the section below for situation 2.

1) The amount you agreed to contribute: ____________________

2) The total contribution of the group: ____________________

3) Project will be implemented: YES [ ] NO [ ]

4) Can the project be provided without your contribution: YES[ ] NO [ ]

5) Your payment: ____

6) Your profit (value-your payment): ________________________

Question 3

Situation	Your value	Proposed contribution	Your decision	Total of other members' contributions
3	6	7	Yes	10

Fill in the section below for situation 3.

1) The amount you agreed to contribute: ____________________

2) The total contribution of the group: ____________________

3) Project will be implemented: YES [ ] NO [ ]

4) Can the project be provided without your contribution: YES[ ] NO [ ]

5) Your payment: ____

6) Your profit (value-your payment): ________________________

Question 4

Situation	Your value	Proposed contribution	Your decision	Total of other members' contributions
4	6	5	Yes	10

Fill in the section below for situation 4.

1) The amount you agreed to contribute: ____________________

2) The total contribution of the group: ____________________

3) Project will be implemented: YES [ ] NO [ ]

4) Can the project be provided without your contribution: YES[ ] NO [ ]

5) Your payment: ____________________

6) Your profit (value-your payment): ____________________

APPENDIX B

Instructions for The Multiple Alternative DCPCM Experiment

B.1 Introduction

This is an experiment in group decision making. During the experiment, you will earn money in "experimental dollars." Your show-up fee has been converted to experimental dollars for you to use during the experiment. At the end of the experiment, your fee and additional earnings will be converted to real dollars and you will be paid in real dollars as you leave.

Today you will participate in four experimental treatments. We will now instruct you on the basic game and treatment I. Following treatment I, there will be another set of instructions that will precede treatment II, a third set of instructions prior to treatment III, and a final set of instructions prior to treatment IV.

The basic game

In this experiment, you will make decisions in a group of five subjects. Together with the four other members of your group, you will decide whether to implement or not to implement a project that provides a benefit to each group member. You must decide how much of your money you will contribute toward the cost of the project.

How you make money

All members of the group receive a benefit when the project is implemented. The project is implemented when the total contribution by all members of your group equals or exceeds the cost of the project. If the project is implemented, you will receive your value for the project. If the project is not provided, you will receive nothing. The levels of contributions you may choose, and the way the payment is calculated based on your offered contribution, will change between treatments. Your profit will be your value minus your payment. Your task is to choose levels of contributions so that you make as much profit as possible.

Values

At the beginning of each period, each subject will learn his or her private value for that period's project. Values are randomly chosen between 5 and 20, with all values equally likely. Different subjects have different values for each period.

Periods

You will play the basic game several times during the experiment, once in each period. At the beginning of each period, all subjects will be randomly assigned to a new group.

Treatment I will have fifteen periods, followed by fifteen periods of treatment II, treatment III and treatment IV.[1] Before playing each treatment for real money, you will do a practice round and a quiz, which will help you understand the rules of the games. The quiz is designed to demonstrate how your decision affects your payment and your profit given the decisions of your group members.

Your final income

You will receive 20 experimental dollars as an initial fund in treatment I and 40 experimental dollars in other treatments, which will reset at the beginning of each treatment. This initial fund will be displayed as your starting total profit. Your profit from each period will be added to this total profit, and your loss will be subtracted from this amount.

[1] This statement was revised before each session to reflect the different ordering of the treatments among sessions.

B.2 Treatment I

In treatment 1, you will learn your value and then be asked to select any amount as your contribution toward the project. If the total contribution of all group members equals or exceeds the project cost, the project will be implemented and you will receive your value. Your profit is your value minus your payment. If the total contribution of all group members is less than the project cost, the project is not implemented. Your contribution will be returned to you, and your payment and profit will be zero for that period.

Decision software

The figure below shows an example of the decision table that you will use to make your contribution decision. The table indicates that in the current period, you are assigned to group number 1, that group 1's project costs 15 experimental dollars to implement, and that your value if the project is implemented is 5 experimental dollars. That is, you will receive 5 experimental dollars if the total contribution from all members of your group is at least 15 experimental dollars. Once you make a decision about how much to contribute, you may type the amount in the box below and click the OK button.

Treatment I: Decision Table

Your group	1
Project cost	15
Your value if the project is implemented	5.00
How much you would contribute toward the cost of the project:	

After all group members have submitted their decisions, your screen will display the results. In the example result table below, the project was not implemented. Despite your contribution of $4, the cost of $15 was not met.[2] As a result, you paid nothing, and received no benefit from the project, giving you zero profit this period.

Treatment I: Result Table

Project cost	15
Your contribution	4.00
Group decision (Yes= project implemented, No=project not implemented)	No
your benefit received	0.00
Your payment	0.00
Your profit	0.00

Summary

1) At the beginning of each period, all subjects are randomly reassigned to groups of five.

2) You learn your private value for the project, with values between $5 and$20 equally likely. Different subjects receive different values.

3) You choose a contribution toward the project.

4) The project is implemented if the total contribution of all group members equals or exceeds the project cost.

5) If the project is implemented, you receive your value; you receive nothing if the project is not implemented.

6) Your payment is equal to your contribution; your profit is your value minus your payment.

[2] All dollars are experimental dollars.

Questions

Since your earnings depend on the decisions you make, it is very important that you understand the procedures and how your earnings will be calculated. If you have any questions, please raise your hand now and the experimenter will answer them.

Practice period

We will now demonstrate this treatment through a practice period. Your earning from the practice periods will not count toward your earnings for the experiment. The project cost is 15 experimental dollars for this practice period.

Practice period 1

1) For practice period 1, everyone will have the same value for the project $5 .

2) For this practice period, everyone will choose a contribution of $4. Click on the contribution box and enter 4.

3) Click the OK button to view the results.

4) Since everyone in your group contributes $4, the total contribution of your group is 5 x 4 =20, which is greater than the project cost $15.

5) Therefore the project is provided, and your payment is $4 and profit is (your value -your payment)= (5 - 4)=1.

6) Click OK to exit the practice screen.

After the practice period, you will be given a quiz, which will help you understand the rules of the game. After the quiz, the real experiment will begin. Your values and costs will be different in each period and also different from those of the other subjects. Once the experiment begins, there will be no communication among subjects, apart from the transmission of responses by the computers.

B.2.1 Quiz

Your group consists of five members. The project cost is 15 experimental dollars. Please answer the following questions based on the rules of treatment I.

Question 1

Your value is 5 experimental dollars and your contribution is 3 experimental dollars. The total of the contributions by the other four members of your group is 10 experimental dollars. Fill in the section below for this situation:

a) The total contribution of the group: ________________

b) Project will be implemented: YES [ ], NO [ ]

c) Your profit (your value - your contribution):____________

Question 2

Your value is 6 experimental dollars and your contribution is 4 experimental dollars. The total of the contributions by the other four members of your group is 12 experimental dollars. Fill in the section below for this situation:

a) The total contribution of the group: ________________

b) Project will be implemented: YES [ ], NO [ ]

c) Your profit (your value - your contribution):____________

B.3 Treatment II
Basic game

From this treatment on, you will decide about the provision of two different projects. Together with the four other members of your group you will decide whether to implement one of the projects, both projects, or none of the projects. You must decide whether you will contribute toward the cost of either, both, or none of the projects.

How you make money

As before, all members of the group receive a benefit when a project is implemented. There are two projects with same provision cost, and your benefit might be the same or different for each project. The projects are labeled as project A and project B. A project is implemented when the total contribution by all the members of your group for that project equals or exceeds the cost of that project. The cost of each project must be met independently in order to be implemented. In other words, depending on the contribution of you and your group members, one project, both projects, or none of the projects will be implemented. If one of the projects is implemented, you will receive your value for that project and make a payment based on your offered contribution. Your profit will be your value minus your payment. If both projects are implemented, then you will receive benefits from both projects and you will pay the sum of the amounts you agreed to contribute for each project. If none of the projects are implemented, you will pay and receive nothing, and all group members' profits will be zero.

The level of contributions you may choose and the way the payment is calculated based on your offered contributions will change between treatments. Your task is to choose level of contributions for each project so that you make as much profit as possible.

Values

At the beginning of each period, each subject will learn his or her private value for that period's projects. Values are randomly chosen between \$5 and \$20 with all values equally likely. Different subjects have different values for each period.

Decision software

The figure below shows an example of the screen that will display the information that you will need to make your decisions. The top part of the figure shows your values and the costs of the two projects, and the rules of games are listed below. The table indicates that in the current period, the cost for both projects is 15 experimental dollars to implement and that your value if project A is implemented is \$ 5 and \$ 8 if project B is implemented. That is, you will receive \$5 if the total contribution from all members of your group for project A is at least \$15 and receive \$8 if the total contribution from all members of your group for project B is at least \$15.

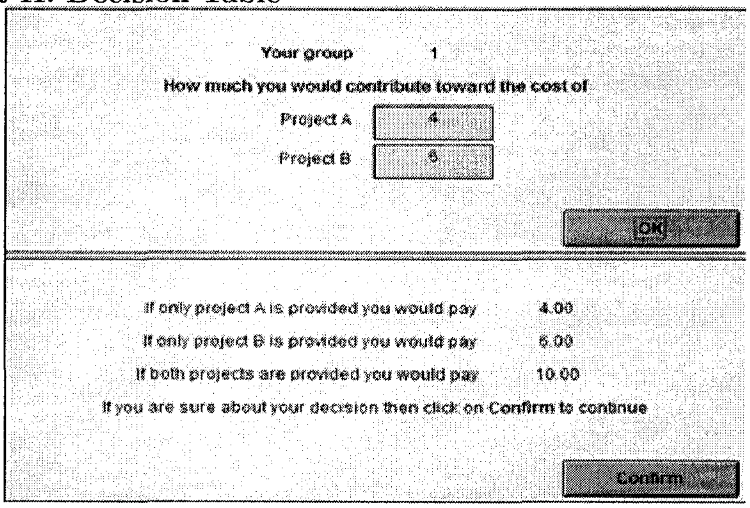
Treatment II: Information Table

Project	Value from a project choice	Project Cost
A	5	15
B	8	15

1.A project is implemented if group contribution for that project equals or exceeds the project cost. If provision costs for both projects are met then both projects are implemented.
2.Your payment toward a project is equal to your contribution if that project is implemented; if both projects are implemented then your payment is the sum of your contributions for each project.
3.If a project is implemented, your profit is your value minus your contribution for that project, if both projects are implemented then your profit is sum of your profits from each project. If no project is implemented, you pay nothing and your profit is zero.

The figure below shows an example of the screen that you will use to make your contribution decisions. The top part of the figure shows your group number. You will type your levels of contribution for each project in the appropriate boxes. Once you make a decision about how much to contribute toward the cost of each project, you may type these amounts in the boxes corresponding to each project and click the OK button. Suppose your contribution is 4 experimental dollars for project A and 6 experimental dollars for project B. The bottom part of the figure displays your contribution decision and asks for confirmation about your decision. If you are sure of your decision, then you click Confirm to see the results of your decision.

Treatment II: Decision Table



After all group members have submitted their decisions, your screen will display the results. In the example result table below, project A was not implemented but project B was implemented. Despite your contribution of \$4, the cost of project A (\$15) was not met; however, the total contribution for project B was at least \$15, and therefore project B was provided. As a result, you paid 6 experimental dollars and received 8 experimental dollars for project B, giving you 8-6=2 experimental dollars profit from project B. Since project A was not implemented you paid or received nothing, giving you 0 profit for project A. Your total profit for this period was 0+2=2.

Treatment II: Result Table

Project	A	B
Project cost	15.00	15.00
Your contribution	4.00	6.00
Group project choice	No	Yes
Your benefit received	0	8
Your payment	0.00	6.00
Your profit	0.00	2.00

Summary

1) At the beginning of each period, all subjects are randomly reassigned to groups of five.

2) You learn your private values for each project, with values between 5 and 20 equally likely. Different subjects receive different values.

3) You choose contributions toward each project.

4) A project is implemented if the total contribution of all group members equals

or exceeds that project's cost. If provision costs for both projects are met, then both projects are implemented.

5) Your payment toward a project is equal to your contribution if that project is implemented; if both projects are implemented, your payment is the sum of your contributions for each project.

6) If a project is implemented, your profit is your value minus your contribution for that project. If both projects are implemented, your profit is the sum of your profits from each project. If no project is implemented, you pay nothing and your profit is zero.

Questions

Since your earnings depend on the decisions you make, it is very important that you understand the procedures and how your earnings will be calculated. If you have any questions, please raise your hand now and the experimenter will answer them.

Practice period

We will now begin one practice period. Your earning from the practice period will not count toward your earnings for the experiment. Each project costs 15 experimental dollars in the practice period.

Practice period 1

1) For practice period 1, everyone will have the same values, \$5 for project A and \$8 for project B.

2) For this practice period, everyone will choose a contribution of \$3 for project A and \$4 for project B. Click on the contribution box and enter 3 for project A and 4 for project B.

3) Click the OK button to view the results.

4) Since everyone in your group contributes \$3 for project A, the total contribution of your group for project A is 5x3=15, which is equal to the project cost \$15. Project A is provided.

5) Since everyone in your group contributes \$4 for project B, the total contribution of your group for project B is 5x4=20, which is greater than the project cost \$15. Therefore, project B is also provided.

6) Your payment is \$3 for project A and \$4 for project B, and profit from A is (your value -your payment)= (5 - 3)=2 and from project B, (your value -your payment)= (8 - 4)=4. Your total profit in this period from both projects is 3+4=7.

7) Now click OK to exit the practice screen.

B.3.1 Quiz

The following questions present two different situations. In each situation, your group consists of five members and each project costs 15 experimental dollars. Column 1 of each table displays your values for each project, column 2 displays your contribution, and column 3 gives total amounts the other four members of your group contributed for each project. Please note that during the experiment, you will not know the other group members' values or contributions. Now answer the following questions based on the information given in the tables.

Question 1

	Your value	Your contribution	Other members' contributions
Project A	7	5	15
Project B	6	2	8

Fill in the section below for this situation:

a) The total contribution of the group: project A: ________ project B:________

b) Project A will be implemented: YES [ ], NO [ ]

c) Project B will be implemented: YES [ ], NO [ ]

d) Your profit from project A (your value - your contribution):________________

e) Your profit from project B (your value - your contribution):________________

f) Total profit this period: ______________________________

Question 2

	Your value	Your contribution	Other members' contributions
Project A	4	3	12
Project B	2	3	13

Fill in the section below for this situation:

a) The total contribution of the group: project A: ________ project B:________

b) Project A will be implemented: YES [ ], NO [ ]

c) Project B will be implemented: YES [ ], NO [ ]

d) Your profit from project A (your value - your contribution):________________

e) Your profit from project B (your value - your contribution):________________

f) Total profit this period: ________________________________

B.4 Treatment III

As in the previous treatment, at the beginning of each period, you will be randomly assigned to a new group of five subjects and learn your private values for each project. However, you will also be given a proposed contribution for each project, which is randomly chosen between 2 and 30. If your decision is to accept the proposed contribution for project A but not for project B, click on button A. If your decision is to accept the proposed contribution for project B but not for project A, click on button B. If your decision is to accept the proposed contributions for both projects click on the both button. If you reject the proposed contribution for each project, click on the none button.

A project is implemented when the total contribution by all the members of your group for that project equals or exceeds the cost of that project. The cost of each project must be met independently in order to be implemented. In other words, depending on the contributions of you and your group members, one project, both projects, or none of the projects will be implemented. If one of the projects is implemented, you will receive your value for that project and make a payment based on your offered contribution. Your profit will be your value minus your payment. If both projects are implemented, you will receive benefits from both projects and you will pay the sum of the amounts you agreed to contribute for each project. If none of the projects are implemented, you will pay and receive nothing and all group members' profit will be zero.

Decision software

The figure on the next page shows an example of the screen that will display the information necessary to make your decisions. The top part of the figure shows your values and the costs of the two projects. The rules of games are listed below. The table indicates that in the current period, the provision cost for both projects is 15 experimental dollars. Your value if project A is provided is 5 experimental dollars and your proposed contribution is 4 experimental dollars. Your value if project B is implemented is 8 experimental dollars and your proposed contribution is 6 experimental dollars. This implies that if the total contribution from all members of your group for project A is at least 15 experimental dollars, you will receive 5 experimental dollars and pay 4 experimental dollars if your decision was to choose A. If the total contribution from all members of your group for project B is at least 15 experimental dollars, you will receive 8 experimental dollars and pay 6 experimental dollars if your decision was to choose B.

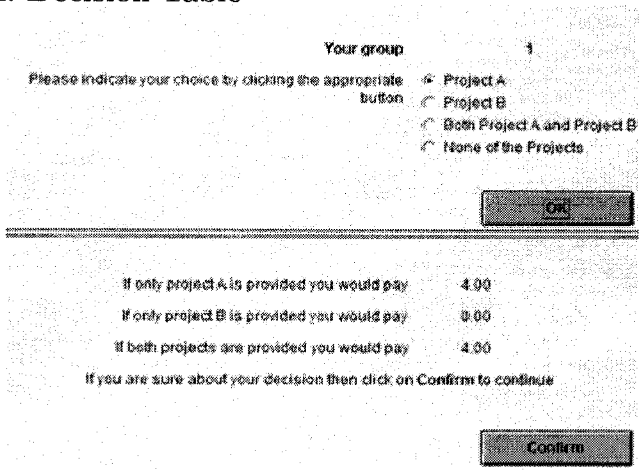
Treatment III: Information Table

Project	Value from the project	Proposed contribution	Project Cost
A	5	4	15
B	8	6	15

1.A project is implemented if group contribution for that project equals or exceeds the project cost. If provision costs for both projects are met then both projects are implemented.
2.Your payment toward a project is equal to your contribution if that project is implemented; if both projects are implemented then your payment is the sum of your contributions for each project.
3.If a project is implemented, your profit is your value minus your contribution for that project, if both projects are implemented then your profit is sum of your profits from each project. If no project is implemented, you pay nothing and your profit is zero.

The figure on the next page shows an example of the screen that you will use to make your contribution decision. The top part of the figure shows your group number. You decide whether to accept or reject the proposed contributions by clicking the appropriate button. The bottom part of this figure displays your contribution decisions toward each project and asks for confirmation about your decision. If you are sure of your decision, then you click Confirm to see the results of your decision.

Treatment III: Decision Table



After all group members have submitted their decisions, your screen will display the results. In the case shown here, you indicated that you would make the proposed contribution of 4 experimental dollars for project A, but you rejected the proposed contribution for project B. The total contribution for project A was greater than the project cost of \$15. As a result, project A was implemented. You received \$5 and made a payment of \$4 for project A giving you 5-4=1 profit this period. The total contribution for project B was less than \$15, so project B was not implemented. Therefore, you received and paid nothing for project B, giving you 0 profit. Your total profit is 1+0=1.

Treatment III: Result Table

Project	A		B	
Project cost		15.00		15.00
Your decision	Yes		No	
Your contribution		4.00		0.00
Group decision	Yes		No	
Your benefit received		5.00		0.00
Your payment		4.00		0.00
Your profit		1.00		0.00

Summary

1) At the beginning of each period, all subjects are randomly reassigned to groups of five.

2) You learn your private values and private proposed contributions for the projects. Different subjects receive different values and proposed contributions.

3) You decide whether to accept the proposed contribution for project A, project B, or both. If you choose to reject the proposed contributions for each project, your contribution is taken to be zero for each project.

4) A project is implemented if the total contribution of all group members equals or exceeds the project cost. If provision costs for both projects are met, both projects are implemented.

5) Your payment toward a project is equal to your contribution if that project is implemented. If both projects are implemented, your payment is the sum of your contributions for each project.

6) If a project is implemented, your profit is your value minus your contribution for that project. If both projects are implemented, your profit is the sum of your

profits from each project. If no project is implemented, your payment and profit are zero.

Questions

If you have any questions, please raise your hand now and the experimenter will answer them.

Practice period

We will now begin one practice period. Your earnings from the practice period will not count toward your earnings for the experiment. Each project costs 15 experimental dollars in the practice period.

Practice period 1

1)You will learn your values and proposed contributions for each project. In this example, everyone has the same values, \$6 for project A and \$7 for project B, and same proposed contributions, \$3 for project A and \$5 for project B. Values and proposed contributions are shown in Rows 1 and 2 of Table B.1.

2) Your decision is to choose A and thus contribute \$5 toward the provision of the project A (rows 3 and row 4).

3) After everyone has entered his or her decision, the group decision is determined. The total contribution for project A is 3x5=15 and for project B is 0 (row 5).

4) The group decision is shown in row 7. Since the total group contribution for project A is equal to the project cost \$15, project A is implemented. However the total contribution for project B is \$0; therefore, project B is not implemented and your profit from project B is \$0 (row 6).

5) Since your chosen alternative is provided, your payment is your offered contribution for project A. However, since project B is not implemented, you receive and pay nothing (row 7).

6) Your profit from project A is, 6-3=3 and your profit from project B is \$0, giving you a total profit of \$3 (row 8).

Table B.1. Practice period 1

Project	A	B
1) Your value	6	7
2) Your proposed contribution	3	5
3) Your decision	Yes	No
4) Your contribution	3	0
5) Group contribution	15	0
6) Group decision	Yes	No
7) Your payment	3	0
8) Your profit	3	0

B.4.1 Quiz

The following questions present two different situations. In each situation, your group consists of 5 members and each project costs 15 experimental dollars. Your value and your proposed contribution under each situation are displayed in columns 1 and 2, respectively. Column 3 of each table displays the total amount the other four member of your group agreed to contribute in each situation. Please note that during the experiment, you will not know the values or contributions of the other group members'. Answer the following questions based on the information given in the tables.

Question 1

	Your value	Proposed contribution	Other members' contributions
Project A	7	5	15
Project B	8	10	11

Your decision is to accept the proposed contribution for project A:

a) The amount you agreed to contribute for project A:______ project B: ______

b) The total contribution of the group for project A: ______ project B: ______

c) Group decision: ____________________

d) Your payment for project A:__________ and for project B : __________

e) Your profit from project A: __________ and from project B : __________

f) Your total profit: ____________________

Question 2

	Your value	Proposed contribution	Other members' contributions
Project A	7	5	15
Project B	8	10	11

Your decision is to accept the proposed contribution for both projects:

a) The amount you agreed to contribute for project A:______ project B:______

b) The total contribution of the group for project A: _______ project B:_______

c) Group decision: ________________________________

d) Your payment for project A:_____________ and for project B: _____________

e) Your profit from project A: _____________ and from project B: _____________

f) Your total profit: ______________________________

B.5 Treatment IV

As in the previous treatment, at the beginning of each period, you will be randomly assigned to a new group of five subjects and learn your private values and proposed contributions for each of the projects. The project provision rule is same as treatment III. However, in this treatment what you pay depends not only on your decision and the group decision, but also on the total contribution by the other four members of your group. If the total contribution of the other four members is sufficient to provide project A without your contribution, then you pay nothing, in spite of your acceptance of the proposed contribution. In this case, your profit is your value. However, if your contribution is required to meet the project cost given others' contributions, then you will pay the amount you agreed to contribute. Here your profit is your value minus your payment. Therefore, you pay only if your contribution makes the difference between providing the project and not providing the project. The project implementation decision for project B, and accordingly your payment and profit, is determined in a similar fashion. On the basis of total group contributions for each project, if both projects are provided then you will receive benefits from both projects, and your payment will depend on the amount you agreed to contribute for each project and the total contribution for each project by your group members. Your profit will be the sum of your profits from each project.

If the total contribution of all group members for each project is less than the respective project costs, then no project is implemented. Your contribution will be returned to you, and your payment and profit will be zero for that period.

Decision software

The figure on page 161 shows an example of the screen that will display the information you will need to make your decisions. The top part of the figure shows your values and the costs of the two projects. The rules of the games are listed in the bottom part. The figure indicates that in the current period, the provision cost for both projects is 15 experimental dollars. Your value if project A is provided is 5 experimental dollars and your pre-assigned proposed contribution is 4 experimental dollars for that project. Your value if project B is implemented is 8 experimental dollars and your pre-assigned proposed contribution is 6 experimental dollars for that project. That is, if the total contribution from all members of your group is at least 15 experimental dollars, you will receive 5 experimental dollars. You will pay 4 experimental dollars if you accept the proposed contribution for project A, and the total amount the other members of your group agree to pay is between 11 and 15 experimental dollars, where 11 is the project cost minus your contribution, giving you a profit of 5-4=1 experimental dollar. If the total contribution of the other members is at least 15 experimental dollars, then the project will be implemented without your contribution, and you will not be required to pay anything in spite of your acceptance of the proposed contribution. If the total contribution by other members is less than 11, then the project cannot be implemented despite your contribution. In this case, your contribution will be returned and the project is not implemented. Similarly, the project implementation decision, your payment, and your profit is determined for project B.

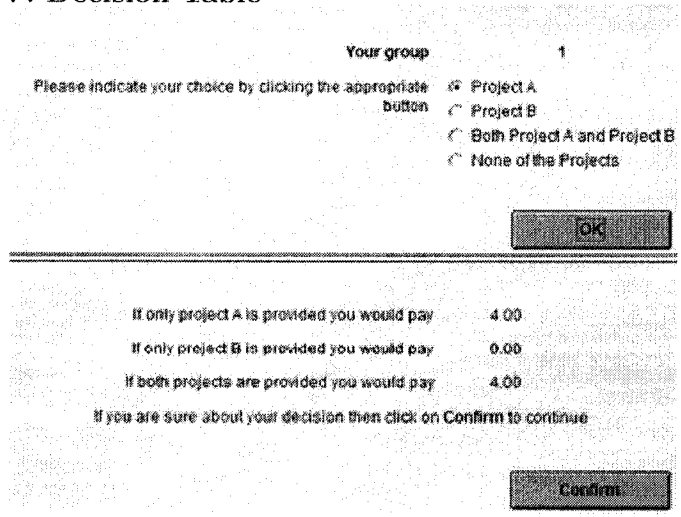
Treatment IV: Information Table

Project	Value from the project	Proposed contribution	Project cost
A	5	4	15
B	8	6	15

1.A project is implemented if group contribution for that project equals or exceeds the project cost. If provision costs for both projects are met then both projects are implemented.
2.Your payment toward a project depends not only on the total group contribution but also on the total contribution by the other members of your group.
3.Your payment is equal to your contribution if your decision makes a difference between implementing the project and not implementing it. If both projects are implemented then your payment is the sum of your contributions for each project.
4.If a project is implemented, your profit is your value minus your contribution for that project, if both projects are implemented then your profit is sum of your profits from each project. If no project is implemented, you pay nothing and your profit is zero.

The figure below shows an example of the screen that you will use to make your contribution decision. The top part of the figure shows your group number. You indicate your willingness to accept or reject the proposed contributions by clicking the appropriate button and then by clicking the OK button. The bottom part of the screen displays your contributions toward each project and asks for confirmation about your decision. If you are sure of your decision, then click Confirm to see the results of your decision.

Treatment IV: Decision Table



After all group members have submitted their decisions, your screen will display the results. In the case shown here, you indicated that you would make the proposed contribution of 4 experimental dollars for project A but make 0 contribution toward project B. The total contribution for project A was greater than the project cost of 15. As a result, project A was implemented. However, the total contribution for project A by other members of your group was between 11 and 15. Therefore, you received 5 and made a payment of 4 for project A, giving you 5-4=1 profit this period. The total contribution for project B was less than 15, so project B was not implemented. You received or paid nothing for project B, giving you 0 profit. Your total profit is 1+0=1.

Treatment IV: Result Table

Project	A	B
Project cost	15.00	15.00
Your decision	Yes	No
Your contribution	4.00	0.00
Group decision	Yes	No
Decision by other members of your group	No	No
Your benefit received	5.00	0
Your payment	4.00	0.00
Your profit	1.00	0.00

Summary

1) At the beginning of each period all subjects are randomly reassigned to groups of five.

2) You learn your private values and private proposed contributions for the projects. Different subjects receive different values and proposed contributions.

3) You decide whether to accept the proposed contribution for project A, project B, or both. If you choose to reject both the projects, then your contribution is taken to be zero for both the projects.

4) A project is implemented if the total contribution of all group members for that project equals or exceeds the project cost. If provision costs for both projects are met, then both projects are implemented.

5) In this treatment what you pay depends not only on your decision and the group decision, but also on the total contribution by the other four members of your group.

6) If the total contribution of the other four members toward the project of your choice is sufficient to provide the project without your contribution, then you pay nothing, in spite of your acceptance of the proposed contribution for the corresponding project. In this case, your profit is your value for that project.

7) However, if your contribution is required to meet the project cost of your choice given the others' contributions, then you will pay the amount you agreed to contribute. Here your profit is your value minus your payment.

8) The project implementation decision, your payment, and your profit for each project is determined in a similar fashion.

9) If both projects are implemented, then you receive benefits from both the projects and make payments toward the costs of both projects based on your decision and other group members' decisions.

10) If both projects are implemented, then your profit is sum of profits from each project. If no project is implemented then your payment and profit are zero.

Questions

If you have any questions, please raise your hand now and the experimenter will answer them.

Practice period

We will now begin one practice period for treatment IV. Your earnings from the practice period will not count toward your earnings for the experiment.

Practice period 1

1) You learn your values and proposed contributions for each project. In this example, everyone has the same values, \$6 for project A and \$7 for project B, and same proposed contributions, \$3 for project A and \$5 for project B. Values and proposed contributions are shown in Rows 1 and 2 of Table B.2.

2) Your decision is to choose both projects, thus your contribution is taken to be \$3 towards the provision of project A and \$5 for project B. (row 3 and row 4).

3) After everyone has entered his or her decisions, the group decision is determined.

4) The total contribution for project A is 5x3=15, and for project B it is 5x5=25 (row 5).

5) The other members' contributions for project A 4x3=12 and for project B 4x5=20(row 6).

6) The group decision is shown in row 7. Since the total group contribution for each project is more than \$15 (the project cost), both projects are implemented.

7) Your payment depends on the other members' decisions. Since the other members' total contribution for project A is between \$11 and \$15, project A cannot be implemented without your contributions(row 8). Your payment is \$3 for project A (row 9). However, since the total contribution for project B is more than the project cost, project B can be implemented without your contributions. Your payment is \$0 for project B (row 9).

8) Your profit from project A is 6-3=3 and from project B it is 7-0=7 (row 10), giving you a total profit of 3+7=10.

Table B.2. Practice period 1

Project	*A*	*B*
1) Your value	6	7
2) Your proposed contribution	3	5
3) Your decision	Yes	Yes
4) Your contribution	3	5
5) Group contribution	15	25
6) Sum of other members' contributions	12	20
7) Group decision	Yes	Yes
8) Project decision by other members	No	Yes
9) Your payment	3	0
10)Your profit	3	7

B.5.1 Quiz

The following questions present two different situations. In each situation, your group consists of 5 members and each project costs 15 experimental dollars. Your value and your proposed contribution under each situation are displayed in a table. Column 3 of each table displays the total amount the other four member of your group agreed to contribute in each situation. Please note that during the experiment, you will not know the other group members' values or contributions. Now, answer the following questions based on the information given in the tables.

Question 1

	Your value	Proposed contribution	Total of other members' contributions
Project A	6	7	15
Project B	8	6	12

Your decision is to accept the proposed contribution for both projects:

a) The amount you agreed to contribute for project A:______ project B:______

b) The total contribution of the group for project A:______ project B:______

c) Group decision:____________________

d) Can project A be implemented without your contribution: YES[ ] NO [ ]

e) Can project B be implemented without your contribution: YES[ ] NO [ ]

f) Your payment for project A:____________ project B:____________

g) Your profit (value-your payment) for project A:______ and project B:______

Question 2

	Your value	Proposed contribution	Total of other members' contributions
Project A	6	7	11
Project B	8	6	12

Your decision is to accept the proposed contribution for both projects:

a) The amount you agreed to contribute for project A:______ project B:______

b) The total contribution of the group for project A:______ project B:______

c) Group decision: ____________________

d) Can project A be implemented without your contribution: YES[ ] NO [ ]

e) Can project B be implemented without your contribution: YES[ ] NO [ ]

f) Your payment for project A:____________ project B:____________

g) Your profit (value-your payment) for project A:______ and project B:______

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