AN EXPERIMENTAL EXAMINATION OF AUCTION MECHANISMS FOR DISCRETE PUBLIC GOODS

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I. INTRODUCTION

In previous research (Ferejohn et al., 1976, 1977, 1979a, 1979b) we have addressed the problem of designing well-behaved choice mechanisms for simultaneously purchasing more than one discrete public good from among several independent alternatives. A "discrete public good" is a public good which is provided in a single, fixed quantity. The initial example that motivated our work (see Ferejohn et al., 1976) was the selection of roughly 30 television programs of fixed duration and content from more than 100 programs that were proposed to public television stations. Several other examples are equally germane, such as the selection of research proposals to be supported by a foundation or the

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decision by partners in a joint oil exploration venture as to the tracts in a field on which to bid. In practice, most collective decisions are posed as a choice among discrete alternatives to simplify the selection process. See Ferejohn et al. (1979b) for more examples.

This paper develops and compares the performance of four collective decision-making procedures for simultaneously determining whether to purchase each of several discrete public goods. In all the procedures the decisions are based upon voluntary "bids" by members of a collective, the sums of which are used to make purchase decisions and allocate cost shares. The procedures differ according to the extent to which they contain specific rules that partially counteract the "free rider" problem, that is, the incentive operating on a member of the collective to misreport the intensities of his or her preferences for the various public goods in order to minimize the proportion of the costs of the public goods that he or she will have to bear. Each of the procedures studied here is based upon a set of experimental institutions designed by Vernon Smith to deal with a different class of problems: the collective choice of a quantity and cost share of a single public good, or the selection of a single, discrete public good from among several alternatives.

The purpose of this research is to examine the performance and implementation features of these institutions when they are applied to the problem of simultaneously selecting multiple, discrete public goods. As discussed below, these mechanisms possess many Nash equilibria, not all of which are efficient. Since economic theory as yet makes no clear-cut predictions as to which equilibria will be attained, the selection of a "satisfactory" decision-making institution may still have an important empirical component. Further, the construction of theory in this area may be assisted by the development of reliable empirical generalizations about the sorts of Nash equilibria that tend to be chosen. For the purpose of generating such empirical information, a series of small-group experiments was performed in which the details of the procedures were systematically varied.

Because of potential variation in environments, it seems plausible that some significant alterations in Smith's procedures may be necessary if his mechanism is to perform successfully in simultaneously selecting multiple public goods. Thus, systematic variation was introduced for two parameters of the experimental environment—the experience of the subjects and the extent of variation in their rank orderings of alternatives. Because marketlike mechanisms for collective decisions are novel and quite complex, experience could be an important factor in determining the performance of these procedures in an experimental setting. Moreover, the complexity of the decision problem can be affected by the preferences of the members of the collective. If experimental subjects are roughly in agreement about which public goods should be selected, the main problem they face in any decision-making environment is how to share costs. Subject groups with more heterogeneous tastes face the additional coordination problem of overcoming comparatively stronger disagreements on what should be purchased, as well as how it should be paid for.

This paper is organized as follows. The next section contains a brief description of the literature relating to the research in this paper, including a formal statement of the procedures examined herein. Section III describes the experiments that we have performed, and Section IV reports the results. Section V provides some incomplete analysis of why the procedures may have differed in their performance in the manner reported in Section VI.

II. ALLOCATION MECHANISMS FOR DISCRETE PUBLIC GOODS

Tideman and Tullock (1976) applied the Clarke-Groves "demand-revealing mechanism" to the problem of specifying incentive-compatible mechanisms for choosing among discrete alternatives. Unfortunately, as shown by Hurwicz (1975) and Green and Laffont (1976), this mechanism will not choose Pareto-optimal alternatives due to the fact that the proceeds collected from individuals will not, in general, exactly offset the cost of providing the public goods.

Thus, to construct mechanisms that will perform "well" in selecting among discrete public goods requires that certain trade-offs be made among various design criteria. In particular, the attractive property of "incentive compatibility"[1] appears to be inconsistent with various other desirable properties. The next logical step, designing mechanisms in which efficient allocations are supported by a Nash equilibrium, has already been pursued.

In a stimulating series of papers, Vernon Smith (1977, 1978, 1979, 1980) has proposed several variants of what he calls the *auction mechanism* for acquiring public goods. In the auction mechanism the agents submit bids indicating their desired quantity of a single public good and the cost shares they would accept. If all the agents agree on the quantity to be provided and on their cost shares, then the public good is provided at the indicated level and with the accepted cost shares. Otherwise, the public good is not produced.

Smith's auction mechanism, with its requirement that agents unanimously agree to the public good quantity and cost shares, provides a partial remedy for the incentive to underreport. Even if everyone else agrees on a quantity and a distribution of cost shares, the failure of just one agent to agree causes no public good to be provided. Smith's modification of the Lindahl mechanism has the property that some efficient allocations are supported by equilibria. Smith (1979) has reported experiments on such processes for environments with one private good and a single public good which can be provided in various integer amounts. He found that in the absence of income effects, the auction mechanism typically provided a quantity of the public good which was consistent with an efficient allocation (although there was usually a revenue surplus), and that agents' bidding behavior could be accounted for by a model predicting that on the final round each agent bids the Lindahl price for the public good. These results are somewhat surprising because the auction mechanism used by Smith has many (Nash) equilibrium strategy configurations, only one of which involves agents bidding their Lindahl prices.

In another paper, Smith (1977) proposed an auction election mechanism for an environment in which there are several mutually exclusive public goods and one private good. Smith found that this mechanism usually chose the "correct" public good (the one maximizing net willingness to pay). Again, these results are difficult to explain because numerous other equilibria exist in which an alternative public good is selected.

One possible explanation for Smith's results is that the preference configuration induced by his payoff structure exhibits "natural" agreement points. In other words, because of his choice of experimental parameters there is no serious "coordination" problem in his public good environments. For example, the net willingness-to-pay function in Smith's initial auction mechanism experiments is single-peaked. Consequently, the search problem may be especially simple for the agents. Indeed, in a recent paper Smith (1980) examined the auction mechanism in an environment in which the choice problem was to select a quantity and cost shares for one public good and in which preferences exhibited income effects. In these experiments the quantity of public good provided was approximately that which corresponds to a Lindahl equilibrium, but the allocation of private and public goods departed significantly from the Pareto set.

Thus we may infer from Smith's work that in certain circumstances the auction mechanism allows the provision of the Lindahl quantity of the public good but that attainment of a Pareto-optimal outcome depends in some unexplained way on the particular environment in which it is implemented. This paper examines its effectiveness in a variety of environments and makes an initial effort to explain its successes or failures in these environments.

In private conversation and correspondence Smith has suggested that a variant of the auction mechanism be used in experiments which deal with several nonexclusive, discrete public goods. And in a preliminary five-person, four-good experiment which he kindly made available to us, this proposed variant did achieve an efficient allocation. In view of the fact that the experimental institutions we have studied (Ferejohn et al., 1979a,b) rarely achieve efficient allocations,[2] we decided to examine closely the auction mechanism with multiple discrete public goods.

The static form of the auction mechanism for discrete public goods may be written as follows. There are n agents and m + 1 commodities, the first m of which are discrete public goods and the last of which is a divisible private good of which each agent holds an endowment w_i. Each agent submits a bid, b_{ij},[3] on each of the public goods, and C_j is the cost of good j. The set $J \subseteq \{1, 2, \ldots, m\}$ is the set of commodity indices for which $\sum_{i=1}^{n} b_{ij} = C_j$, and indicates the collection of public goods which are to be produced. The i^{th} agent then is required to pay $\sum_{j\varepsilon J} b_{ij} = \sum_{j\varepsilon J}(C_j - \sum_{k\neq i} b_{kj})$ for the package of produced public goods. Each of the agents is assumed to hold a linear preference ordering over the commodities. Thus, the i^{th} individual's preference R_i, between the (m + 1) vectors, $x = (x_1, \ldots, x_m, x_{m+1})$ and $y = (y_1, \ldots, y_m, y_{m+1})$, can be represented as a vector $v_i = (v_{i1}, v_{i2}, \ldots, v_{im+1})$ where $v_{im+1} = 1$ and $xR_iy \Leftrightarrow \sum_{j=1}^{m+1} x_jv_{ij} \geq \sum_{j=1}^{m+1} y_jv_{ij}$, where x_{m+1} is the quantity of private good and $x_j = 1$ if commodity $j = 1, \ldots, m$ is included in bundle x and $x_j = 0$ otherwise. In this case the i^{th} individual's marginal willingness to pay for good j is represented by v_{ij} and we further assume that $C_j > v_{ij} > 0$ for all i and j.

Under the above assumptions it is easily seen that any bid configuration for which for all i and for $j = 1, \ldots, m$

$$b_{ij} = \begin{cases} C_j - \sum\limits_{k\neq i} b_{kj} & \text{if } C_j - \sum\limits_{k\neq i} b_{kj} > v_{ij}; \\ 0 & \text{otherwise} \end{cases}$$

is a Nash equilibrium and conversely. Of course, as mentioned earlier, there are usually many such equilibria, including the configuration $b_{ij} \equiv 0$ for all i and j. It can be seen that some of the bid configurations of this form support a Pareto-optimal allocation, so that in a weak sense the auction mechanism is "weakly incentive compatible." That is, at least some Nash equilibria are contained in the Pareto set.

The critical features of the static auction mechanism are these (Smith, 1978):

1. *Collective excludability:* If the collectivity fails to purchase a commodity, each individual is excluded from enjoying it.

2. *Unanimity:* For a commodity to be purchased all individuals must bid their cost shares (i.e., $b_{ij} = C_j - \sum_{k \neq i} b_{kj}$).
3. *Budget balance:* The cost of the purchased commodities equals the sum of the bids.

While these features produce Pareto-optimal Nash equilibria, they also possess numerous nonoptimal equilibria as well. Because of the existence of multiple equilibria, the agents must have some opportunity to "coordinate" their strategies in such a way that a particular equilibrium can be attained. This is achieved by implementing the auction mechanism as a dynamic adjustment procedure.

Several distinct adjustment processes could be interpreted as methods which implement the static auction mechanism. We have chosen to work with a simple basic adjustment process and to experiment with variations in several of its characteristics as suggested by Smith (1980). The processes examined in this paper all have the property that for each agent the tax assessment does not exceed the initial endowment (nonbankruptcy). To ensure nonbankruptcy, we require that for each agent the sum of bids not exceed the agent's initial endowment.[4] Other constraints on bids could serve the same purpose, but any such alternative is equivalent to this requirement applied after some redistribution of the endowments.

Steps 1 and 2 describe the canonical adjustment procedure which was employed for all the experiments:

1. In each round t, each agent i submits a bid vector on all commodities, $(b_{i1}^t, \ldots, b_{im}^t) = b_i^t$ where $\sum_{j=i}^{m} \leq w_i$. For every commodity, the sum of bids on that commodity is made public at the end of the round. The experimenters then indicate which of the commodities would be produced if the given bid vector were final (i.e., the commodities for which $\sum_{i=1}^{n} b_{ij}^t \geq C_j$).
2. The process can stop in either of two ways: (a) reach round ten; or (b) every agent submits identical bid vectors for two consecutive rounds.

Several alternative allocation rules were appended to the basic adjustment process:

3a. *Voluntary contribution and unanimity procedure:* If $(b_1^t, \ldots, b_n^t)$ is a final-bid configuration, stopping was achieved by rule 2b, and J is the set of commodities to be produced, then the payoff to agent i is $\sum_{j \varepsilon J} (v_{ij} - b_{ij}^t) + w_i$. If stopping was achieved by rule 2a, then the agent receives a small fixed sum p_0.

3b. *Voluntary contribution procedure:* All agents receive $\sum_{j\varepsilon J}(v_{ij} - b^t_{ij}) + w_i$ regardless of how the final-bid configuration was achieved.

3c. *Rebate and unanimity procedure:* If $(b^t_i, \ldots, b^t_n)$ is the final-bid configuration and stopping was accomplished by 2b, then the payoff to agent i is

$$\sum_{j\varepsilon J}(v_{ij} - b^t_{ij}) + w_i + \sum_{j\varepsilon J}\left\{\frac{b^t_{ij}}{\sum_k b^t_{kj}}\left(\sum_k b^t_{kj} - C_j\right)\right\} \tag{1}$$

$$= \sum_{j\varepsilon J}\left(v_{ij} - \frac{b^t_{ij}}{\sum_k b^t_{kj}} C_j\right) + w_i.$$

If stopping occurs because of 2a, then the agent received a small fixed sum of money p_0.

3d. *Rebate procedure:* Agents receive payments calculated according to (1) regardless of how stopping occurs.

These adjustment and allocation rules depart from the static auction mechanism in several ways. First, for a commodity to be purchased in the experiments, we require only that $\sum_{i=1}^n b_{ij} \geq C_j$, whereas the auction mechanism requires equality. Second, the cost share for agent i is then either $b_{ij} \geq C_j - \sum_{k\neq i} b_{kj}$ in 3a and 3b, or b_{ij} minus a rebate in 3c and 3d.[5] In the auction mechanism, agents always pay $b_{ij} = C_j - \sum_{k\neq i} b_{kj}$. Thirdly, the sum of the bids an agent may submit must not exceed his budget.

Allocation rules 3a and 3b are called voluntary contribution procedures because they amount to a very common method of cost sharing for public goods—the "fund drive." A commodity (a church, recreation center, or university research program) can be purchased at some price C. A group then inaugurates a campaign to raise the money. If enough money is gathered, the commodity is purchased; otherwise the contributions are returned to the donors.

A problem with voluntary contribution procedures is that agents might be expected to understate their preferences in part because the procedure can produce a surplus and this surplus is simply discarded. Rules 3c and 3d (rebate procedures) distribute the surplus of bids over costs on each commodity to the agents in proportion to their final bids on the commodity. This feature is supposed to reduce incentives to understate willingness to pay for the public goods, although it should not be expected to eliminate underbidding entirely. Moreover, overbids on a commodity are rarely consistent with the existence of a Nash equilibrium in this case.[6]

Rules 3a and 3c incorporate the requirement that agents unanimously signal their agreement on a proposed allocation by repeating their bids. Unless such agreement is explicitly achieved, the agents receive a small compensation for their time (p_0 = \$3.00) and no commodities are purchased. In procedures 3b and 3d no penalty is assessed if unanimous agreement is not achieved, and the payoffs are made according to the final-bid configuration.[7]

The diversity of dynamic implementations of the auction mechanism permits several questions to be addressed. First, how well do any of the processes perform in an environment with multiple discrete public goods? In particular, are efficient or "nearly" efficient allocations achieved in this setting? Second, does the use of the "rebate" rule affect the performance of the mechanisms? Finally, does the use of the unanimity requirement affect the performance of the mechanisms?

Two additional hypotheses are of interest. First, would the unanimous agreement experiments (3a and 3c) exhibit less strategic understatement of bids (i.e., "free riding") than the other experiments? Second, would the voluntary contribution rules (3a, 3b), which confiscate overbids, discourage high bids (relative to 3c and 3d) and therefore inhibit the achievement of efficient outcomes? There is little theoretical support for any particular answer to these questions, but they seem to require some response if one is to succeed in implementing procedures of the type examined here.

III. DESCRIPTION OF THE EXPERIMENTS

The principal purpose of the experiments reported here is to study the effect of the different allocation rules given above. In addition to examining experimental auction processes with each of the allocation rules, two additional control variables were introduced. First, two distinctly different preference configurations were used. Second, "experienced" subjects (subjects who had participated in earlier experiments) were used in some of the experiments.

The two preference, endowment, and cost configurations that were employed are given in Tables 1 and 2. The preference configuration in Table 2 was employed by Smith (private communication) in a pilot experiment with the auction process for discrete public goods. Table 1 exhibits more diversity in the relative attractiveness of the alternatives among the agents than does Table 2. In Table 2, bundle BCD is weakly preferred by everyone and strongly preferred by everyone except agent 5 to any other feasible bundle. Thus, the principal difference between the two configurations is that the location of Pareto-optimal allocations seems much more straightforward for the data in Table 2 than for those

Table 1.

		Agent 1	2	3	4	5	*Cost*
Commodity	A	3.00	1.80	2.40	1.20	4.80	6.00
	B	1.20	3.60	6.00	1.80	3.00	9.00
	C	4.80	8.40	6.60	2.40	5.40	12.00
	D	3.60	6.00	3.60	3.00	7.20	10.50
	E	1.20	5.40	3.60	4.20	1.80	7.50
Endowment		3.30	8.25	9.90	4.95	6.60	

in Table 1.[8] Hence, we shall refer to Table 1 as the "difficult" configuration and Table 2 as the "easy" configuration.

The decision to use experienced subjects was based on practical considerations relating to the possible implementation of procedures of the sort examined here. Allocation mechanisms for public goods are sufficiently unfamiliar to most people that they may have to undergo a period

Table 2.

		Agent 1	2	3	4	5	*Cost*
Commodity	A	1.00	1.00	1.00	1.00	1.00	2.00
	B	5.00	5.00	5.00	5.00	5.00	4.00
	C	2.00	3.00	4.00	5.00	6.00	3.00
	D	8.00	6.00	4.00	2.00	1.00	1.00
Endowment		3.00	2.00	2.00	1.00	1.00	

during which they acquaint themselves with the operation of the procedures before they fully understand how the mechanism operates. In most potential applications the agents will eventually obtain such experience through repeated participation. Thus, the performance of the institutions with experienced subjects may be the more interesting set of data. And because very little seems to be known in economic choice experiments about these matters, we decided to employ "experience" as a control variable.

Twenty-nine five-person experiments were conducted. The subjects were mostly recruited from the undergraduate student populations of Caltech and Pasadena City College. In no case were experienced and inexperienced subjects mixed. The design of the alternative allocation rules proceeded sequentially, and so subjects were not randomly assigned to experiments. Table 3 classifies the experiments that were run.[9]

The experiments proceeded as follows. The instructions (see Appendix) were read to the subjects and an opportunity was given for questions. When the subjects appeared to be familiar with the instructions, they were asked to submit a list of bids on the commodities. The bids were then collected and the sum of bids on each commodity were posted on the blackboard in the front of the room. Although cost information was freely available to the subjects, in order to avoid confusion the experimenters indicated at the end of each round which commodities, if any,

Table 3. Design of Experiments* Allocation Rules

		3a	*3b*	*3c*	*3d*
Table 1 Preferences	Experienced	1	1	1	0
	Inexperienced	2 (1)**	2 1	3 (1)**	5 (2)**
Table 2 Preferences	Experienced	1	1	1	1
	Inexperienced	2	2	2	4

Notes:
* Entries are the number of observations in each cell.
** Experiments deleted from analysis due to misunderstanding of instructions by subjects.

had bid sums exceeding purchase cost. The subjects were then asked to submit a new set of bids. This procedure was repeated until one of the stopping conditions, 2a or 2b, was achieved.

Once a stopping condition was met, the final payoffs were computed according to the particular allocation rule being employed for the experiment. The subjects were then paid in cash and dismissed.

In the case of 4 of the 29 experiments (marked in Table 3), the experimenters concluded that one or more of the subjects did not understand the instructions. As one would expect, these experiments involved only inexperienced subjects. These cases are eliminated in the following analysis to insure that technical defects in communicating the instructions do not obscure the results. All the tables reported below were examined for the full set of 29 experiments, and any differences in results are noted where appropriate.

IV. THE EFFECTS OF THE ALTERNATIVE ALLOCATION RULES ON THE PERFORMANCE OF THE EXPERIMENTAL INSTITUTIONS

In an earlier paper (Ferejohn et al., 1979a), total payout to subjects was used as the measure of the efficiency of an experimental mechanism. We are somewhat uncomfortable with this measure because it is an adequate index of efficiency only if subjects may make side payments among themselves. In particular, the total payout measure is especially sensitive to the performance of the institution with respect to the payoffs captured by the one or two participants with the biggest stakes in the decision. Outcomes with relatively poor performance by this measure may, indeed, be on the Pareto frontier because they provide the maximal payout to some participants. For this reason, additional indicators of allocative efficiency are used here.

The most direct measure is simply to indicate whether a particular experiment terminated at an efficient (Pareto-undominated) allocation. Not surprisingly, given the difficulty of the coordination problem faced by the subjects, only 6 of the 29 experiments resulted in efficient outcomes. In several experiments a group chose a package of public goods that was consistent with an efficient allocation, but overbid for these goods. Given the uncertainty faced by the subjects about how their peers would respond to an attempt to reduce overbids, situations with overbids seem quite natural. Thus, a second measure of the efficiency of an experimental outcome counts an allocation as "efficient" if the only allocations dominating it consist of the *same* public good bundle but with different cost-sharing arrangements. This occurs with the voluntary contribution mechanism if the sum of the bids exceeds the cost. In this sense, 5 more "efficient" outcomes (a total of 11) were observed.

A useful property of a measure of efficiency is that it give some indication of *how* inefficient the inefficient allocations were. One measure, admittedly ad hoc but easy to compute, is the percentage of the number of packages of public goods for which there existed some cost-sharing arrangement such that each subject could be made better off by securing that package. This measure, D, took values between 0 (for "efficient" outcomes) and 1.0 (for very inefficient outcomes), with experiments that have lower scores exhibiting better performance than those with higher scores.

Finally, as an alternative measure of efficiency, the percentage of the total possible payout to the subjects P is also computed. Whereas this measure is subject to the same criticisms as the total payout measure discussed above, it coincides fairly closely to the D measure. For example, in 5 of the 10 experiments in which the measure D took the value of 0 for efficient outcomes, the subjects received 100% of the total possible payout. In the other 5 experiments, all of which were with the voluntary contribution mechanism, the subjects earned at least 83% of the total possible payout by purchasing an efficient public good bundle but by collectively bidding more than the cost of the bundle. This measure takes on values between 0 and 1.0. If the 4 experiments in which we suspect that at least one subject failed to grasp the instructions are eliminated, the values are between .164 and 1.0.

Table 4 contains the results of experiments with all six decision-making institutions. Results from auction procedures that use inexperienced and experienced subjects are pooled in the table. Some experiments involving the last two institutions mixed experienced and inexperienced subjects, so that the results can not be conveniently divided and compared.[10]

The results in the table motivate the more detailed analysis that follows. The results from the four types of auction institutions neatly bracket the results of the SPC and pivot procedure, raising the possibility that a well-designed version of one of the former could outperform the latter two. Second, the particular allocation rule used in an auction procedure appears to affect performance. Moreover, there appear to be interaction effects between the design of institutions and the heterogeneity of the preferences of the group that uses them, as well as other environmental features. Unfortunately, the number of experiments is too few and the experimental design too sparse to test for these effects. Nevertheless, the results in the table are sufficiently striking to bear underlining—as well as to justify more experiments. Unanimity appears to do an effective job of promoting agreement on a relatively efficient allocation when members of the group have heterogeneous tastes, but the straight "char-

Table 4. Performance[a] of Six Institutions

Institution	*Heterogeneous Preferences*[b]			*Homogeneous Preferences*[b]		
	Number of Experiments	*P-Measure*	*D-Measure*	*Number of Experiments*	*P-Measure*	*D-Measure*
3a. Voluntary Contributions and Unanimity	2	.79	.06	3	.80	.08
3b. Voluntary Contributions	3	.46	.61	3	.99	.00
3c. Rebate and Unanimity	3	.95	.01	3	.78	.07
3d. Rebate	3	.49	.61	5	.73	.09
Station Programming Cooperative	5	.60	.22	—	—	—
Discrete Pivot[10] Procedure	8	.57	.33	—	—	—

Notes:

[a] P-measure is proportion of maximal total payoff, D-measure is proportion of feasible allocations that Pareto-dominate the final allocation.

[b] Heterogeneous preferences are shown in Table 1, and homogeneous preferences are shown in Table 2. The Station Programming Cooperative and Discrete Pivot Procedures were not run on homogeneous preferences.

ity drive"—the classic case for the "free rider problem"—performed virtually perfectly when preferences were homogeneous. We have no solid explanation for this result.

To analyze in greater detail the effect of the various institutional and environmental factors on performance, the relationship between both of the efficiency measures and the various experimental controls was estimated using the Tobit method (see Tobin, 1958).[11] This method was appropriate because the measures of efficiency are limited dependent variables. The results using percent of maximum feasible payout as the performance measure are shown in Table 5. These results were obtained from using a Tobit procedure with 1 − P as the dependent variable, where the lower limit on the observations of this dependent variable was taken to be zero.[12] Utilizing the .10 significance level for the two-tailed tests, the hypothesis that an independent variable affects performance is accepted for three of the four control variables.

In a Tobit model, the locus of expected value estimates is nonlinear and is given by

$$E(1 - P \mid X) = B'XQ(B'X/s) + sZ(B'X/s), \qquad (2)$$

where Q(x) is the value of the cumulative, unit-normal distribution function at x; Z(x) represents the value of the unit-normal probability function at x; and B'X is the product of the coefficient vector with a vector of observed values of the independent variable. The estimate of standard deviation of the normal random variables with mean zero is s, which is

Table 5. The Effects of Experimental Controls on 1 − P

	B	
Preferences		
(= 0 if Table 1,	−.203*	
= 1 if Table 2)	(.106)	N = 25
Unanimous Consent		
(= 1 if 3.a or 3.c	−.188*	s = .245
= 0 if 3.b or 3.d)	(.107)	
Experience		
(= 1 if experienced subjects	−.225*	
= 0 otherwise)	(.121)	
Rebate		
(= 1 if no rebate	.087	
= 0 with rebate)	(.103)	
Constant	.449**	
	(.126)	

Notes:
* Significant at the .10 level
** Significant at the .01 level

assumed to account for the differences between subject groups which are not accounted for in the independent variables (see note 11).

The effect of the control variables can be calculated from the estimates in Table 5 and Equation (2). For example, inexperienced subjects using the difficult preference configuration without a unanimous consent rule capture, on average, E(P) = (1 − E(1 − P)) = 54.8 percent of the total possible payout. Imposing a unanimous consent rule on inexperienced subjects with the same preference structure increases the expected percentage payoff to 72.1 percent.

While the statistical analysis supports the proposition that experienced subjects using the more homogeneous preference configuration (Table 2) and unanimous consent would operate at almost full efficiency (96.4 percent), this conclusion is tentative. The small number of experiments, especially using experienced subjects, cannot support a statistical test for interaction effects.

Table 6 reports the Tobit estimation results when the measure of performance is the percentage of feasible bundles of public goods for which there exists a distribution of cost shares such that they Pareto-dominate the final allocation in the experiment. Since D is also a limited dependent variable, this relationship is estimated using a Tobit procedure in which D is the dependent variable and has a lower limit of zero.[13]

In this case, the same three of the four control variables significantly affect the performance criterion based on the number of dominating allocation. On average, an inexperienced collective with the more heterogeneous set of preferences and without the unanimous consent rule

Table 6. The Effects of Experimental Controls on D

	B	
Preference	−.391**	
	(.143)	
Unanimous Consent	−.387**	N = 25
	(.145)	
Experience	−.418**	s = .283
	(.168)	
Rebate	−.044	
	(.130)	
Constant	.598***	
	(.166)	

Notes:
** Significant at .05 level
*** significant at .01 level

settles on an allocation which is dominated by 59.9 percent of the feasible packages. Alternatively, using the same preferences, an inexperienced group which operates under a unanimous consent rule will settle on an allocation which is dominated by 24.8 percent of other possible equilibria.

These regressions suggest that the particular allocation rule utilized in implementing an auction mechanism affects its performance. The critical aspect of the allocation rule is whether the agents are required to signal unanimous consent on a final allocation by repeating their bids. Whether the overbids are returned to agents (rebate) appears to have no affect on performance.

The two additional control variables, preference configuration and experience, may also have significant impacts on performance. For all institutions, the Table 2 preferences yield somewhat better performance than those in Table 1. Somewhat nebulous post hoc examination of the histories of the experiments provides a possible explanation for this phenomenon. Groups using Table 2 preferences settle more quickly on the final list of commodities to be purchased than do the other groups. They then devote the remaining rounds to attempts at relatively minor adjustments in cost shares.

Groups of experienced subjects performed better than inexperienced groups by one of our efficiency measures. While this result may not be a surprise, it seemed to us almost as plausible (before running the experiments) that experienced subjects might be more likely to try to engage in strategic behavior to influence the bids of others than would the inexperienced subjects, thereby inhibiting institutional performance.

V. DISCUSSION AND A POSSIBLE INTERPRETATION

While it is of interest to know which characteristics of an institution (such as the unanimous consent feature or the presence of rebate) or of its environment (experience, preference configuration) affect its performance, also of interest is *how* and *why* these factors influence outcomes. Apparently various aspects of an institution and an environment give subjects greater or lesser incentives to stabilize their bids. And in any adjustment procedure of the sort studied here, unless agents stabilize their bids, the institution evidently cannot end up performing very well. Maximum performance is achieved in cases in which agents are able to eliminate many alternatives from consideration early in the process, to concentrate bids only on the commodities that eventually turn out to be purchased, and to eliminate overbids.

To examine these ideas empirically, a measure of the volatility of bids in an experiment was used as an indicator of how much predictability there was in bidding behavior. The measure utilized was

$$\frac{1}{TNM}\sum_i \sum_t \sum_j \left| \frac{b_{ij}^t}{w_i} - \frac{b_{ij}^{t-1}}{w_i} \right| = V,$$

where T is the number of rounds, N is the number of agents, M is the number of goods, and V is simply the average variation in bids for each experiment. The idea is that the various aspects of an institution or its environment might affect volatility, which in turn would affect performance by affecting coordination possibilities. Alternatively, certain aspects of an institution or its environment might directly affect performance. A variety of simple recursive systems was estimated in which volatility and performance (measured in either of the ways reported in the previous regressions) are endogenous variables, but in which volatility is antecedent to performance. Given this conjectured relationship, a regression in which volatility is the dependent variable was estimated using ordinary least squares. These results are shown in Table 7.

Table 8 gives the results of regressions in which the percent of total possible payout was used as a dependent variable, while Table 9 shows the results when the dependent variable is the percentage of feasible bundles that Pareto-dominates the final allocation. The relationships presented in both of these tables were estimated using the Tobit method for limited dependent variables. In all cases volatility and preference configuration were independent variables. Each of the other three independent variables was also tried with volatility and performance, as shown.

These results suggest that features of both the environment and the institution affect the performance of the auction mechanism for discrete

Table 7. Effect of Controls on Volatility

	B	
Preference	−.002	R^2 = .30
	(.009)	
Experience	−.019**	N = 25
	(.01)	
Unanimous Consent	−.018**	
	(.009)	
Rebate	.009	
	(.009)	
Constant	.074	

Note:
** Significant at .05 level

Table 8. Influence of Volatility on Percentage of Total Possible Payout

Independent Variables	Regression Number *1*	*2*	*3*	*4*
Volatility	−5.767**	−5.711**	−6.568***	−6.581***
	(2.266)	(2.200)	(2.184)	(2.169)
Preference	.152	.157	.142	.142
	(.098)	(.097)	(.098)	(.098)
Unanimous Consent	.095			
	(.106)			
Experience		.126		
		(.116)		
Rebate			−.006	
			(.097)	
Constant	1.013***	1.013***	1.114***	1.112***
	(.180)	(.167)	(.149)	(.146)
s	.229	.226	.233	.234
N	24	25	25	25

Notes:
*** Significant at the .01 level
** Significant at the .05 level

Table 9. Influence of Volatility on Percentage of Feasible Dominating Bundles

Independent Variables	Regression Number *1*	*2*	*3*	*4*
Volatility	6.680**	7.157**	9.843***	9.125**
	(3.008)	(3.094)	(3.363)	(3.272)
Preference	−.334**	−.336**	−.299*	−.292*
	(.140)	(.144)	(.149)	(.150)
Unanimous Consent	−.312**			
	(.148)			
Experience		−.363*		
		(.177)		
Rebate			−.172	
			(.148)	
Constant	−.014	−.090	−.305	−.332
	(.237)	(.227)	(.221)	(.223)
s	.286	.296	.219	.326
N	24	25	25	25

Notes:
*** Significant at the .01 level
** Significant at the .05 level
* Significant at the .10 level

public goods. Using either performance measure, a decrease in volatility significantly improves performance and, from Table 7, we know that both the use of experienced subjects and the unanimous consent rule significantly decreases volatility. One explanation for this result rests on the information problem faced by the subjects. In experiments with experienced subjects or ones in which there is a strong incentive to reach agreement (i.e., the unanimous consent experiments), subjects generally do not vary their bids much from round to round because they realize that by so doing they would make it difficult for others to predict their behavior. The resulting stability in bids in such experiments allows the subjects to settle on the commodities that will be bought and their cost shares relatively easily.

Except for the rebate property which appears to have no impact at all on either performance or volatility, the other control variables significantly affect performance directly as measured by D but seemingly have no effect on performance as measured by P. Experience, unanimous consent, and the preference configuration appear to have a direct influence in locating an allocation which is dominated by fewer of the feasible equilibria but do not seem to affect the percentage of the total possible payout which a subject of either of these groups achieves. As discussed previously, both of these measures are arbitrary exensions of the "efficiency" criterion and do not perfectly coincide in ranking the outcomes of the experiments. Thus the differences reported in Tables 8 and 9 may well be due to these arbitrary differences in the ranking of outcomes.

In general, the procedures reported here—especially the unanimity requirement—perform surprisingly well. Even though the choice problem is quite difficult, the average performance level is fairly high. For example, 15 of the 29 experiments (or 25 if we eliminate experiments in which at least one subject did not understand the instructions) resulted in allocations that are dominated by fewer than 10% of the feasible bundles. If the standard for success is that fewer than 20% of the bundles dominate the outcome, then 23 experiments were successful. Even though there are many Nash equilibria—especially in the unanimous consent experiments—the subjects still managed to settle on a bundle that was relatively efficient most of the time. We cannot explain why this should be so. Indeed, given the complicated environments in our experiments, this tendency is perhaps even more puzzling than the similar results obtained by Smith.

APPENDIX

To conserve on space the instructions used in all different mechanisms will not be reproduced here. Instead, the set of instructions for rule 3a using Table 1 preferences are shown and footnotes will indicate any variations that were used with other rules. An asterisk (*) will indicate where changes were made when using Table 2 preferences to accommodate the fact that only four items were available to the group.

An Experiment in Group Decision Making

This is an experiment in the economics of group decision making. The instructions are simple and if you follow them carefully and make good decisions, you may earn a considerable amount of money which will be paid to you in cash at the end of the experiment.

You are a member of a group that must decide which of various items to purchase. Group discussion, however, will *not* be permitted under any circumstances during the experiment. If a particular item is purchased by the group, it has a certain dollar *value* to you. If you turn to the last page of these instructions you will see recorded there the dollar *values* of each of the five* items (A, B, C, D, E). This value is the amount the experimenter will pay to you if this item is purchased at the *conclusion* of the experiment. Each group member may have a different set of values and these are to be considered private. You are asked not to reveal these numbers to anyone during the experiment. You will also find the cost of providing each of the five* items to the group on the last page of this handout. Since these are the costs *to the group* this information is the same on each member's handout.

The method by which the group will make its purchase decisions is as follows. There will be a sequence of no more than 10 periods. In each period you will be asked to privately select and communicate to the experimenter how much you would be willing to pay or "bid" in order to obtain each item. At the end of each period the experimenter will list the total amount bid for each item by all members of the group on the blackboard in the front of the room. He will also list those items for which the total amount bid exceeds the cost of providing them. If an item appears on this list on the *final period* of the experiment, it is "purchased." If an item is not on this list in the final period, even if it appeared on the list in an earlier round, it will not be purchased.

Bidding Rules

In your folder you will find a set of bidding forms. At the beginning of each period you will be asked to fill out one of these forms. The information which you must fill out on each form includes: your member number, the period number, and the amount you are willing to pay for each item. The bids which you submit must satisfy the following requirements:

1. In each period you may bid on as many of the five* items as you wish.

2. Your bid on any item must be in multiples of quarters. For example, \$.25, \$1.50, \$1.75, \$3.25, or \$6.00 are valid bids, but \$.37, \$1.61, \$2.42, or \$3.18 are not valid.
3. You are not permitted to bid a negative amount for any item, although you may bid zero.
4. In each period, the total amount which you may bid on all items must not exceed your allotted budget. You will find your budget is recorded on the last page of these instructions. This is the amount of money the experimenter has credited to you. You are not required to bid your entire budget.

Here is an example of a list of bids which satisfy the above rules:*

Item	*Your Bid*	
A	\$30.00	
B	\$20.00	
C	\$15.00	Budget – \$100.00
D	\$12.50	
E	\$22.50	
Total Amount Bid	\$100.00	

Computing Your Payoff[14]

Your payoff will be determined by the list of bids you have submitted in the final round and the list of items purchased in that round. An item is purchased only if the sum of the bids on that item is at least as great as the cost of providing it to the group. If an item is purchased, you will pay what you bid on that item.

For example, if you bid \$2.00 on item A, the total amount bid on item A is \$8.00, and its cost is \$6.00, the item would be purchased and you would have to pay \$2.00 for item A.

If an item is not purchased, you will not have to pay anything for that item.

Your total payoff is computed by the following method. It is the sum of the values of the purchased items plus your unspent budget. Your unspent budget is the amount of your budget that remains after you pay what you bid on items that were purchased. The following example will help illustrate the method of calculating payoffs.

Example. To assist you in computing your payoff we will work through an example* together. Suppose that the following information is furnished to you at the conclusion of a period.

ITEMS A, C AND D ARE PURCHASED

Item	Value	Bid Totals	Your Bid	Cost
A	$5.00	$ 9.00	$3.00	$ 6.00
B	$2.00	$ 4.00	$0.00	$ 9.00
C	$6.00	$12.00	$2.00	$12.00
D	$3.00	$12.00	$4.00	$10.50
E	$2.50	$ 4.00	$1.00	$ 7.50

Your Budget = $10.00

Compute your payoff if the experiment were to terminate at this stage.[15] The experimenter will come around and check your work.

Termination

You will have a maximum of *10* periods to decide on your purchases. This procedure will terminate prior to this only if all group members agree to stop by submitting identical bids for two consecutive periods. If the group agrees to stop, you will be paid according to the above rules. However, if the group has failed to agree to stop by the end of period 10, you will each be paid $3.00.[16]

Once the experiment is terminated, payoffs will be made. You should compute the amount which is owed to you by the experimenter and it will be paid to you in cash at the conclusion of the experiment. Feel free to earn as much cash as you can. Are there any questions?

If there are no further questions, we may begin. Remember that you may not communicate with each other during the experiment. Please decide upon your initial bids and record them on your bidding form. Remember that these bids are *private* and should not be revealed to the other group members. Please pass these forms to the experimenter when you have recorded your bids.

NOTES

1. A mechanism is incentive-compatible if truthful revelation of preferences is an optimal strategy for each agent given any configuration of preferences. This implies that each agent has a dominant strategy.

2. In Ferejohn et al. (1979a) we reported the results of an experimental analysis of two mechanisms in a laboratory setting. One was a laboratory analog of the choice mechanism (the Station Program Cooperative or SPC) utilized by the Public Broadcasting System. In the SPC, stations vote yes or no for each proposed program and costs are shared only by stations who vote yes. The procedure is iterative, and stations may change their votes from iteration to iteration as the number of yes votes—and hence the cost share to a station—changes. The procedure ends when all agents vote the same way in two consecutive rounds. The other procedure we have studied was a modified version of a procedure

found in the literature on incentive compatibility (see Green and Laffont, 1977). In this procedure, tax shares for each agent depend on the costs of the purchased goods and the bids of other agents. Neither of these mechanisms "performed" much better than the other in an experimental setting, although a moderate edge had to be given to the SPC mechanism employed on two grounds: it seemed to converge more quickly and therefore to be somewhat less costly to operate, and it appeared to exhibit less variance in outcomes.

3. Alternatively, agents could submit bids on distinct packages of public goods. This, however, is ruled out for practical reasons since agents would be required to submit 2^{m-1} bids.

4. This nonbankruptcy constraint may introduce a significant income effect on each member of the collective at the boundary. To see this let J be the set of commodities to be produced, then each agent's preference ordering may be represented as

$$\sum_{j \in J} v_{ij} + \max\left[0, w_i - \sum_{j \in J} t_{ij}\right],$$

where t_{ij} is the cost share of each produced commodity allocated to agent i under an allocation rule. For any positive private good allocation, the above preference ordering is linear in income and thus no income effects are present. However, when the private good allocation nears zero, this "kink" in the preference ordering induces a sharp income effect.

5. Rules 3b and 3d are actually variants of Lindahl mechanisms. Under fairly general conditions in an environment with discrete public goods, Lindahl mechanisms, like auction mechanisms, possess Nash equilibrium Pareto-optimal allocations.

6. For the rebate procedures a bid configuration for which the bid total for a commodity exceeds its costs can be an equilibrium only if exactly 1 individual has bid on the commodity.

7. One feature of the experiments reported in our earlier work makes them noncomparable to the current experiments. Namely, subjects were told that the experiments would terminate either if all agents submitted identical bids for two consecutive rounds or if the round number exceeded some predetermined (secret) number (always 10).

8. Table 1 preferences have several bundles for which there exist cost sharing arrangements supporting a Pareto optimum. For example, BC, CD, CDE, and BDE all have many cost-sharing arrangements which yield a Pareto optimum.

9. Obviously the experimental design is incomplete. More experiments per cell would have produced more reliable statistical results. Moreover, a greater range of variation in the environmental control variables is clearly desirable. Larger groups and a greater number of options could also be subject to experimental investigation. The difficulty with a more complete experimental design is, of course, its expense and complexity. To add another two-way treatment—such as independent variation of income distribution and preferences—doubles the number of experiments. While we are proceeding with additional experiments, we believe these preliminary results provide useful information about the problem of designing decentralized, marketlike process for group decision making.

10. In a subset of these experiments subjects made purchasing decisions subject to the restriction that at most *one* commodity could enter or leave the list of those provisionally purchased in each round.

11. The Tobit method for estimating relationships for limited dependent variables is appropriate whenever the observed dependent variable cannot take on values below a certain threshold level. In the cases considered here either measure of performance, say $1 - P$, is a limited dependent variable with a lower limit of 0. Let Y be a linear combination of the independent variables $B'X$ to which $1 - P$ is related by hypothesis. Subject groups may differ from each other in their behavior regarding the efficiency measure for reasons for which differences in the independent variables do not fully account. These differences

are taken to be random and are representable by ϵ, a normal random variable with mean 0, and variance σ. The performance of a group is assumed to be given as:

$$\begin{aligned} 1 - P &= 0 && \text{if } Y - \epsilon < 0; \\ 1 - P &= Y - \epsilon && \text{if } Y - \epsilon \geq 0. \end{aligned}$$

12. The appropriate estimation procedure here is the two-limit Tobit method since P cannot be negative or exceed 1. Because there were no observations for which $P = 0$, the single-limit Tobit method produces identical estimates as the two-limit method (see Rosett and Nelson, 1975).

13. As in the previous estimation, a single-limit Tobit method was used since there were no observations for which $D = 1$.

14. When the rebate rule was used, this section reads as follows:

Computing Your Payoff. Your payoff will be determined by the list of bids you have submitted in the final round and the list of items purchased in that round. An item is purchased only if the sum of the bids on that item is at least as great as the cost of providing it to the group. If the total amount bid on an item exceeds its cost, then the excess of bids over the cost will be rebated to members in proportion to their bids. If you wish to compute your rebate on an item, you may do so in the following manner:

$$\text{Your rebate} = \frac{\text{your bid}}{\text{total bid}} \times (\text{total bid} - \text{cost}).$$

For example, if you bid \$2.00 on item A, the total amount bid on item A is \$8,00, and its cost is \$6.00, the item would be purchased and your rebate would be:

$$\text{Rebate} = \frac{\$2.00}{\$8.00} \times (\$8.00 - \$6.00) = \$.50.$$

To find your cost share for a purchased item subtract your rebate from your bid on that item. In this example your cost share for item A is \$2.00 − \$.50 = \$1.50. If an item is not purchased your cost share is zero.

Your total payoff is computed by the following method. It is the sum of the values of the purchased items plus your unspent budget. Your unspent budget is the amount of your budget that remains after you pay your cost shares. The following example will help illustrate the method of calculating payoffs.

15. When the rebate rule was used, this sentence read: "Compute your cost share for each item and your payoff if the experiment were to terminate at this stage."

16. When unanimity was not required, this paragraph read as follows:

> You will have a maximum of *10* periods to decide on your purchases. This procedure will terminate prior to this only if all group members submit identical bids for two consecutive periods.

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