A TATONNEMENT MECHANISM FOR ALLOCATING PUBLIC GOODS: THEORY AND EXPERMENTS

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I. Introduction

Since the development of the Groves-Ledyard mechanism (Groves and Ledyard, 1977), there has been considerable theoretical interest in synthetic mechanisms for allocating public goods, but there has been little work on the implementability of these mechanisms. In this paper we develop a new mechanism for implementing a Lindahl equilibrium. This mechanism retains the efficiency and implementability of a simple Lindahl process, complete with the appropriate Lindahl prices, while eliminating most of the incentive problems inherent in previous versions of that process. In addition, one possible lump-sum taxation scheme has demand-revelation as a Nash equilibrium when participants have identical, single parameter quasilinear preferences.

This paper also describes a series of experiments designed to test the implementability of the mechanism. Most of the experiments are conducted on the PLATO interactive computer system, which acts as both a Walrasian auctioneer and the passive medium of information transfer and display.[1] In general, the results show quite dramatically that the mechanism almost always leads subjects to choose an outcome which is either exactly equal to the Lindahl equilibrium or very close to it. This result is found for both experienced and inexperienced subjects, using three different kinds of preference profiles. One particular preference profile has consistently yielded free riding with voluntary provision and a high incidence of convergence failure under the the Smith auction mechanism (see Isaac, McCue,

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[1] Four experiments (HR1-HR4) were run "by hand," using an IBM portable computer to recompute prices and lump-sum taxes each iteration.

and Plott, 1985; and Banks, Plott, and Porter, 1985).

The paper is organized as follows. Section II presents a brief review of the literature on synthetic public goods allocation mechanisms and Section III presents a brief outline of the Binger-Hoffman (B-H) mechanism. Section IV then describes the experimental design, Section V presents the experimental results, and Section VI concludes.

II. Review of the Literature

Since the work of Lindahl (1919) and Wicksell (1896), the inefficiency of competitive markets in allocating public goods has generated theoretical interest in developing alternative allocation mechanisms. Moreover, recent experimental work on voluntary provision (Isaac, McCue, and Plott, 1985; Isaac, Walker, and Thomas, 1984; Banks, Plott, and Porter, 1985; Isaac and Walker, 1984, 1985) indicates quite strongly that voluntary provision fails to generate efficient outcomes when subjects can revise their responses in light of others' responses, unless they can also communicate directly with one another. Lindahl (1919) envisioned a tâtonnement process similar to the Walrasian tâtonnement process, but the incentive properties of such a mechanism are not encouraging. In addition to the incentive problem of all tâtonnement mechanisms identified by Hurwicz (1972), the Lindahl tâtonnement mechanism provides a strong incentive for participants to underreveal relative to their true demands for the public good. Another tâtonnement process, developed by Malinvaud, Dreze, and de la Vallee Poussin (the MDP procedure) (Dreze and de la Vallee Poussin, 1971; Fujigaki and Sato, 1981; Green and Schoumaker, 1980; Henry, 1979; Malinvaud, 1971; Roberts, D.J., 1979, 1983; Sato, 1981; Schoumaker, 1979a,b; Truchon, 1980, 1983; Tulkens, 1978) is still not immune to strategic manipulation by participants.

The theoretical literature on implementation has focussed more on iterative bidding and taxation procedures than on procedures designed to converge to Lindahl prices. For example, in the Groves-Ledyard mechanism (Groves and Ledyard, 1977), participants indicate marginal quantities of the public good they wish to see provided. They are then taxed according a quadratic tax rule which is largely based on the responses of the other participants. At a Nash equilibrium, participants' responses satisfy the Lindahl-Samuelson conditions. In the Smith auction, (Smith, 1979, 1980) participants propose quantities of the public good and bid to contribute to its provision. The auctioneer then proposes taxes which are functions of the mean quantity proposed and the other participants' bids. One of the possible Nash equilibria of the Smith auction has participants paying the equivalent of Groves-Ledyard taxes (Smith, 1979).

The mechanism we have developed combines some of the valuable properties of both the Groves-Ledyard and the Lindahl mechanisms: it generates Lindahl prices at a truthful Nash equilibrium, it is difficult to manipulate, and it is easy to implement, because it requires participants to make only one simple decision per iteration. It uses the tâtonnement framework and retains the simplicity of the original Lindahl mechanism. But, it significantly reduces the incentive to misrepresent, by adopting an adjustment mechanism with incentive properties similar to the Groves-Ledyard mechanism. Moreover, for one particular lump-sum taxation scheme, demand revelation is a Nash equilibrium for some preference profiles.

III. The B-H Mechanism

At each iteration of the mechanism, the auctioneer sends each participant (i) a private message, telling the participant his or her personalized price for the public good (z_i) and a net personalized lump-sum tax or transfer (τ_i). The personalized prices always sum to the marginal cost of providing the public good (p_y), and the net taxes and transfers always sum to zero. Given the messages received, each participant is asked to respond by proposing a quantity of the public good (Y_i), which implies the he or she will contribute

$$(z_i Y_i + \tau_i)$$

towards the group's purchase of the public good. The auctioneer then checks for a public goods equilibrium, defined as all participants proposing the same quantity of the public good.

The process begins by assuming equal personalized prices for all participants, which are then adjusted at each iteration. The mechanism uses a common parameter (α), defining a linear function, (αt), which goes through the origin. Each participant is assigned a value for t along that function (t_i). The t_i's are defined so that each participant's personalized price (z_i) is equal to αt_i and the sum of the z_i's equal p_y.

In some of the experiments reported in this paper, the lump-sum taxes and transfers were also constructed from α and the t_i's. Each participant received a transfer or subsidy equal to:

$$\frac{\alpha t_i^2}{2}\,,$$

and paid a tax (T_i),

$$T_i = \sum_{j \neq i} \frac{\alpha t_j^2}{2(n-1)} .$$

Other experiments were conducted using a simpler taxation scheme, that has the property that truthful revelation is a Nash equilibrium if all participants have the same, single-parameter, utility function.[2] That taxation scheme is:

$$\tau_i = \frac{\sum_{j \neq i} Y_j}{n-1} (p_y - nz_i)$$

The adjustment mechanism uses an indirect excess demand method. At each iteration, after each participant has sent his or her Y_i message, the auctioneer calculates a variable (y_i) for each individual as the difference between Y_i and t_i:

$$y_i = Y_i - t_i$$

The excess demand method is that t_i is treated as i's demand in a hypothetical auxiliary market and the sum of the other participants' y_j's is treated as the corresponding supply. An "equilibrium" in this hypothetical market would have:

$$t_i = \sum_{j \neq i} y_j , \quad \forall i.$$

i's demand = supply from $j \neq i$

Such an "equilibrium" constitutes a fixed point of the adjustment process and implies a public goods equilibrium as well. To see that, note

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[2] We developed the new taxation scheme when we were about half finished running the experiments. We decided to switch to using it because of its improved incentive properties. We did, however, re-run one set of experiments, which had been conducted using the original taxation scheme. The results were not affected by the taxation scheme for that preference profile.

that:

$$y_i = Y_i - \sum_{j \neq i} y_j, \quad \forall i$$

at a fixed point. Thus:

$$Y_i = \sum_{i=1}^{n} y_i = Y, \ \forall i.$$

There is also an equilibrium value for α consistent with the fixed point. By construction, the personalized prices always sum to p_y:

$$p_y = \sum_{i=1}^{n} z_i = \sum_{i=1}^{n} \alpha t_i = \alpha \sum_{i=1}^{n} \sum_{j \neq i} y_j = \alpha(n-1) \sum_{i=1}^{n} y_i$$

$$= \alpha(n-1)Y.$$

Therefore:

$$\alpha = \frac{p_y}{(n-1)Y} \text{ in equilibrium.}$$

If the responses do not generate such an equilibrium, the auctioneer adjusts personalized prices as a function of "excess demand" in the hypothetical auxiliary market and then normalizes so that they sum to the marginal cost of the public good:[3]

$$z_i = \frac{\alpha[t_i + \gamma(t_i - \sum_{j \neq i} y_j)]}{\sum_{k=1}^{n} \{\alpha[t_k + \gamma(t_k - \sum_{r \neq k} y_r)]\}} p_y, \quad 0 < \gamma \leq 1$$

This formulation ensures that

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[3] The parameters used to adjust the personalized prices and α seem to be fairly robust, although they were not chosen as a result of any optimization procedure. We considered several possible ways of endogenizing these parameters, but found that the incentive properties of the mechanism were adversely affected.

$$p_y = \sum_{i=1}^{n} z_i .$$

It also ensures that the only way i's response affects his or her own z_i adjustment is through the introduction of the participant's y_i in the normalization value in the denominator. The proportionate z_i given in the numerator depends only on the y_j's from the other individuals, and on t_i, which is not a decision variable.

The auctioneer also adjusts α as a function of the difference between the previous value and the value consistent with the average of the participants' Y_i responses.

$$\hat{\alpha} = \alpha + \delta \left[\alpha - \frac{p_y}{\frac{(n-1)}{n} \sum_{i=1}^{n} Y_i} \right], \; 0 < \delta \leq 1$$

Next, the t_i's to be used to calculate the taxes and subsidies are defined so that $z_i = \alpha t_i$:

$$\hat{t}_i = \frac{z_i}{\hat{\alpha}}$$

Thus,

$$\sum_{i=1}^{n} \hat{\alpha}\hat{t}_i = p_y$$

These $\hat{t}_i$'s allow calculations of new T_i's, S_i's, and τ_i's:

$$T_i = \frac{\hat{\alpha}}{2(n-1)} \sum_{j \neq i} (\hat{t}_j)^2, \text{ using the simple taxing scheme}$$

$$S_i = \frac{\hat{\alpha}(\hat{t}_i)^2}{2}$$

$$\tau_i = T_i - S_i$$

In the next round, each participant chooses Y_i, conditional upon these new parameters:

$Y_i = Y_i(\phi_i; z_i, \tau_i)$, where

ϕ_i = a vector of behavioral parameters.[4]

Choosing Y_i implies a contribution of:

$C_i = z_i Y_i + \tau_i$.

Therefore, by induction, at each iteration m:

$$y_i^m = Y_i^{m-1} - t_i^{m-1}$$

$$z_i^m = \frac{\alpha^{m-1}[t_i^{m-1} + \gamma(t_i^{m-1} - \sum_{j \neq i} y_j^m)]}{\sum_{k=1}^{n} (\alpha^{m-1}[t_k^{m-1} + \gamma(t_k^{m-1} - \sum_{r \neq k} y_r^m)])} p_y$$

$$\alpha^m = \alpha^{m-1} + \delta\left[\alpha^{m-1} - \frac{p_y}{\frac{(n-1)}{n} \sum_{i=1}^{n} Y_i^{m-1}}\right]$$

$$t_i^m = \frac{z_i^m}{\alpha^m}$$

$$T_i^m = \frac{\alpha^m}{2(n-1)} \sum_{j \neq i} (t_j^m)^2$$

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4 While the components of the vector of behavioral parameters is not spelled out in any mechanism, we see them as being conditioned on the information which participants receive about the mechanism. That is, they may behave differently if they know exactly how the mechanism works, if they get different kinds of feedback during the operation of the mechanism, or if they participate several times and gain experience. Thus, in a sense, it is this aspect of the model that is being explored experimentally. One of the questions we are asking is how different ways of presenting the mechanism to participants affect their decisions at this stage of the process.

$$S_i^m = \frac{\alpha^m (t_i^m)^2}{2}$$

$$\tau_i^m = T_i^m - S_i^m$$

$$Y_i^m = Y_i(\phi_i;\ z^{m-1},\ \tau_i^{m-1})$$

$$C_i^m = z_i^m Y_i^m + \tau_i^m$$

The equilibrium conditions are:

$$Y_1^m = \ldots = Y_n^m$$

$$t_i^{m-1} = \sum_{j \neq i} y_j^m, \quad \forall i,$$

where,

y_i^m = i's imputed supply in the hypothetical auxiliary market

t_i^{m-1} = i's imputed demand in the hypothetical market

The process continues until the responses are close enough to a fixed point by some a priori margin of precision.

The auctioneer begins this process by assigning equal personalized prices and assessing zero net taxes and transfers. Thus, the initial messages from the auctioneer are:

$$z_i^0 = \frac{p_y}{n}, \ \forall i \ \text{ and}$$

$$\tau_i^0 = 0, \quad \forall i.$$

Participants respond to these initial messages at round 0:

$$Y_i^0 = Y_i(\ \phi\ ;\ \frac{p_y}{n},\ 0)$$

The auctioneer then calculates initial values for α and t_i on the basis of the average Y_i^0 response:

$$\alpha^0 = \frac{p_y}{\frac{(n-1)}{n} \sum_{i=1}^{n} Y_i^0}$$

$$t_i^0 = \frac{z_i^0}{\alpha^0} = \frac{p_y}{n\alpha^0}$$

Thus,

$$y_i^0 = Y_i^0 - t_i^0$$

The first round then proceeds as described above, except that

$$\alpha^1 = \alpha^0 \text{ and}$$

$$y_i^1 = y_i^0$$

IV. Experimental Design

A. PLATO, Computerized Experiments

1. Inexperienced Subjects

At each iteration, subjects are presented with a table showing the payoff, in "PLATO dollars", associated with various combinations of x and Y. The displayed Y values are always integers from 0 to 74, inclusive, and always yield non-negative profits.[5] This is the only point in the experiment that requires information on utility functions. The utility function is used only for the purpose of displaying each participant's profits and is not used in any centralized calculations. Implementation in a naturally-occurring choice situation, where participants' utility functions would not be known, could be easily accomplished by displaying the level of expenditure, $z_i Y + \tau_i$, associated with each potential integer proposal. Each par-

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[5] The Instructions, including sample payoff sheets, are given as Appendix B.

ticipant would then choose a Y_i (implying a $ expenditure) at each iteration, based on his or her naturally-occurring (non-induced) preference ordering. This is equivalent to setting each participant's initial allocation (x_i^0) equal to zero and then displaying the x values as payments. Moreover, by allowing a much larger range of responses than, for example, the PLATO version of the Smith auction, our program is easily adapted to any particular range of options which might be considered in a naturally-occurring implementation.

Each subject is then asked to respond with an integer quantity of Y. The conversion rate from PLATO dollars to U.S. dollars is given in the instructions and reiterated upon reaching a potential final allocation in the experiment. We used PLATO dollars in order to get separation in the payoffs around the optimum value and still keep total payoffs about $15-$20 per subject.[6]

At each iteration, subjects were presented with new payoff tables, reflecting the adjusted personalized prices and lump-sum taxes and transfers. In an implementation with non-induced preferences, participants would also be presented with new tables, based on their new taxes at each iteration, showing how much they would have to pay for each possible quantity of the public good. The adjustment mechanism was not explained to the subjects, but they were told that their responses would not directly affect the adjustment of their own parameters. In an implementation with non-

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[6] See Forsythe, Palfrey, and Plott (1982) for a discussion of the use of artificial currencies in experimental markets. See, also, Cox, Smith, and Walker ( ) and Harrison ( ) for a recent debate on artificial currencies and possible biases from small differences in actual payoffs.

induced preferences we would explain the mechanism to participants and allow them to practice with it before using it for an actual decision. The stopping rule of the mechanism is a unanimous "yes" vote on a unanimously proposed Y.

Subjects in these experiments were undergraduates at the University of Arizona, recruited largely from principles of economics classes. They were all inexperienced in this particular experiment, although some of them had participated in other PLATO experiments, such as the double auction. They were promised a $3 participation fee, which they were paid at the beginning of each experiment, plus whatever additional earnings they made. Subjects typically made between $10 and $25 in this experiment, which typically lasted about 2 hours when 4 successive decisions are made.

Each experimental group made between 2 and 4 group decisions in sequence. Most made 4 decisions, but a few took so long that our time limit of 2 hours ran out before the last decision (in one case 2 decisions) could be made. The first three experiments involved 4 decisions each, using a set of preference profiles which generate the same linear demand curves used by Isaac, McCue, and Plott (1985) and Banks, Plott, and Porter (1985).[7] Basically, this involved inducing a quadratic utility function over the private good (x) and the public good (Y), with no income effects in the choice of the public good to generate the subject payoff tables. This set of preference was chosen first because it was shown to generate nearly zero provision of the public good with a voluntary provision mechanism (Isaac,

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[7] The sets of preference profiles used in the various experiments are reproduced in Appendix A.

McCue, Plott, 1985) and to contribute to a high proportion of failures to converge under the Smith auction (Banks, Plott, and Porter, 1985). What makes these parameters tough is that there are two groups of participants with very different demands for the public good who have to agree on a quantity. In addition, with our design, the disagreement outcome of 0 units of Y actually gave each subject about $4 per decision ($16 over the whole experiment), making agreement far less attractive than if they thought they would make $0 or some small amount if they disagreed. We reasoned that if our mechanism failure to converge with these "tough" parameters, then we should not continue with our testing.

When these first 12 decisions yielded near 100% efficiency (see Section V), we ran another set of 4 experiments (3 with 3 decisions each and one with 4) in which we used the same kind of quadratic utility function (which we call the IMP utility function); only now we gave each of the ten participants a different implicit linear demand curve and designed it so that at least one person has a Lindahl price arbitrarily close to zero at the Lindahl equilibrium. We reasoned that these parameters should be even tougher than the original ones. Now, each of the 10 participants has a different demand curve: some with relatively high Lindahl prices and some with quite low (even 0) Lindahl prices. In addition, we maintained the feature that the disagreement outcome was worth about $4, so that subjects could earn $12-$16 without cooperating. Finally, since all these first 7 experiments were run at the old Economic Science Laboratory in the Science Library and the next set were run in the new laboratory in the Economics Building, we also run two replications of this set later on in the new laboratory.

The next set of experiments used 6 subjects with CES and Cobb-Douglas induced utility functions, one of which was a replication of one of the utility functions Smith used to test the Smith auction (Smith, 1979, 1980).[8] Another used the same Cobb-Douglas utility coefficients, but scaled up the incomes to multiply the group choice by 5. The other two utility functions were CES. In one design there were 3 different sets of CES parameters and in the other design each of the six subjects had a different set of CES parameters. Moreover, in the last design elasticities of substitution varied between .5 and 4 and one person's Lindahl price was nearly zero. Those with elasticities of substitution greater than 2 earned positive profits at the disagreement outcomes, while others earned $0. This introduced a further degree of divergence among the subjects. The final set of computerized experiments with inexperienced subjects replicates the original IMP experiments using the new tax rule in the new experimental laboratory.

Table 1 summarizes the design and location of each of the experiments described verbally above. Experiments 001-003 replicate the IMP parameters and experiments 004-007 plus 013-014 use all different IMP-type (i.e., linear demand generating) parameters. Experiments 008-011 combine CES and Cobb Douglas parameters. The replications of the Smith (1970, 1980) parameters are in experiments 008c, 009c, 010b, and 011b, each of which has

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[8] Our parameters appear slightly different because we transformed the Cobb-Douglas utility coefficients to sum to 1 and we adjusted the incomes to account for the fact that our mechanism leads to a different final distribution of income than Smith's does. These changes left the ordinal properties of the utility functions unchanged and insured that the equilibrium of our mechanism was the same as the equilibrium of his.

an equilibrium of 9 units of the public good. Finally, experiments 031-034 replicate the original IMP parameters using the new tax scheme, which had just been developed at that time.

2. Once-Experienced Subjects

Having successfully shown that the mechanism generates Pareto optimal allocations with inexperienced subjects, we brought back as many previous subjects as we could recruit to participate in a new set of experiments. This time we explained the operation of the mechanism to the subjects and gave them full information about their payoff functions. As inexperienced subjects, they had only been given payoff information for points along their budget lines. Now, we supplemented the information on their PLATO screens with printouts of their payoffs for all integer combinations of X and Y from (0,0) to (75,75). We also instructed them in how to interpret the points along their budget lines they saw on the PLATO screen. Each subject was given a ruler with which to draw those budget lines on their payoff grids. The experiments then proceeded as before, with each group making either two or three decisions in sequence. Each group consisted of six subjects, each of which was given a differnt CES payoff function.

Table 2 outlines the design and location of each of the PLATO experiments with once-experienced subjects. Experiments 015-019 involved two decisions and 020-023 involved three.

3. Twice-Experienced Subjects

Despite continued success with once-experienced subjects, we were still concerned about the possibility of failure if experienced participants started trying to manipulate the mechanism. Consequently, we recruited subjects who had participated twice before to participate in an experiment in which we instructed them in manipulation and then paid them on the basis of one real decision. Table 3 outlines the design and location of each of these experiments.

After being given a chance to re-read the instructions, if they wished, they were told that they would participate in several trial decisions of 10 rounds each that probably would not end in agreement. They would not be paid for these trial decisions. After the trials they would participate in a decision for which they would be paid. That decision could take as long as 25 rounds.

During the first trial decision we suggested to the participants that they try putting in responses that were higher than the responses that would maximize individual profits. The groups that took our advice then started to converge on a relatively high group decision. They were then encouraged to record what their profits would have been if the group had reached agreement on a high number. During the second trial decision we suggested they try relatively low responses, and during the third trial decision we suggested they respond truthfully. Each time we encouraged them to record their final profits. Where time permitted, we allowed one or two more trial decisions, in which they were encouraged to try any strategies they wished. For each trial decision, we reinitialized be giving each subject the same CES payoff function he or she had had the previous trial. For the decision on which they were paid, they used the same payoff functions and we gave no suggestions for how they might respond.

Since each subject had a different CES payoff function, this exercise should have shown them the possible benefits from deception. Those with stronger preferences for the public good than the average for the group would have discovered that their payoffs tended to be higher if the group chose relatvely high numbers. Those with weaker preferences for the public good than the average for the group would have discovered that their payoffs tended to be higher if the group chose lower numbers. And, those with preferences near the average for the group would have discovered that they made higher payoffs when the outcome was inbetween.

B. Hand-Run Experiments, Inexperienced Subjects

Our final concern was that the computerized task, itself, encouraged subjects to appear to myopically maximize. The profit-maximizing choice was on the screen each iteration, perhaps making subjects feel stupid if they did not maximize each round. For the last series of experiments we took the decision out of the PLATO lab altogether. We programmed an IBM PC to recalculate the taxes for each participant each round. The experiment was then run by hand, except for a portable computer to do the recalculation each round. Each subject was given complete payoff information and had to calculate his or her potential profits associated with different quantities of the public good proposed by subtracting the taxes that would be paid at that quantity from the gross profit.

Each round, each subject was asked to submit a proposed quantity of the public good on a slip of paper, which was collected by the monitor. If they agreed on a quantity we took a vote and paid them their gross payoffs minus their contributions if they all voted yes. If they did not agree, we loaded the responses into the portable computer, recomputed taxes, and printed them out for each participant. Twenty-five rounds were allowed to reach agreement. Table 4 outlines the design and location of the hand-run experiments.

V. Experimental Results

A. PLATO, Computerized Experiments

1. Inexperienced Subjects

Table 5 reports the computerized experimental results for inexperienced subjects. Notice first that, out of 50 decisions, only three groups failed to agree, for an overall failure rate of 6%. However, despite three failures, the average percentage deviation from the optimum is only 9.08% and, on average, 94% of the optimal quantity is provided and the subjects collected 98% of the potential payoffs at equilibrium.

Moreover, as Table 3, indicates, 2 of the groups which failed to reach agreement did so because of a failure to vote unanimously on an agreed-upon choice. When we just consider agreements, the failure rate falls to 2% and the average percentage deviation from the optimum falls to 5%. 98% of the optimal quantity would have been provided on average and 98% of the equilibrium payoffs would have been distributed.

Tables 4-6 compare failure rates and efficiency measures for the different preference profiles. Notice that the original replication of the IMP parameters and the CES and Cobb-Douglas parameters lead to nearly 100% efficiency on every dimension. All but one of the replications of the Smith (1979, 1980) parameters, for example yields exactly the Lindahl quantity and the one other yields only one unit less.

The only problem seems to come with the IMP-type parameters where each subject has a different utility function (Table 5). All three failures occurred under that design, two in the same experiment. In fact, that experiment only involved two decisions because the subjects took so much time trying to make those two decisions that our time ran out. Moreover, in each of the experiments where a failure occurred, it occurred because one individual either consistently vetoed the group choice or consciously tried to manipulate the mechanism. Whether this particular set of preferences is particularly vulnerable to such behavior, or whether unusual subjects happened to arrive in these particular experiments requires further investigation. Despite these failures, it is important to note that the subjects in these experiments still collected overall 96% of the equilibrium payoffs. This is, of course, partly due to the fact that the surplus from cooperation is relatively small in this particular design.

To conclude our discussion of the results, we note several general observations about how subjects behaved in these experiments. First, they did not simply maximize profits blindly from the beginning. This is evident from a comparison of Figures 1 and 2. Figure 1 shows a graph of computer-simulated, profit-maximizing responses for the replication of Isaac, McCue, and Plott (1985). This shows that if everyone maximizes profits, one group chooses 28 and the other chooses 18 on the first round. On the second round they all agree on 24. Figure 2 shows the actual responses for decision 1 of the second experiment (decision 002a). As Figure 2 shows, they eventually do agree on 24, but they beginning by exploring a number of nonmyopic types of responses (some above and some below 24). By round 11 it is clear that the outcome will be either 23 or 24, but it takes 6 more rounds before everyone agrees on 24. Even then, at round 18 one person vetoes the group choice, and then decides to go along at round 19. In fact, it is common for one individual to exercise his or her right to veto (especially on the first decision of an experiment) and then come back to agree with the group choice at the same outcome the next round. It is as if some individuals just want to test out their individual power to veto the group choice.

Figure 3 shows the importance of learning and hysteresis in these experiments. This figure shows the responses of the same group of subjects on their third decision with the same parameters (decision 002c). First, note that the first-round responses are all profit maximizing. After 2 or 3 decisions we found that subjects came to trust that the mechanism would lead them to a group decision if they maximized profits. Thus, they would often all profit maximize on the first round after 2 or 3 decisions. They do not always continue to profit maximize, however, as Figure 3 shows. On the second round at least one person did not choose 24, even though by the third decision in this experiment it was already clear that 24 was going to be the outcome. It appears as though more experienced subjects profit maximize the first round of a new decision, perhaps in order to see clearly within what range the outcome will fall. Then, they may try other strategies for several rounds, before returning to the original range of responses and letting the mechanism bring them in.

Second, once a group is moving in the direction of coming to agreement, there is often a strong hysteresis effect. Sometimes it works to improve the efficiency of the mechanism and sometimes it works against it. In the experiments in which we repeated the same parameters for several decisions, subjects learned quickly that 24 was the equilibrium, even though we did not tell them their parameters would be the same. This allowed them to try out other strategies in early rounds of later decisions and then return to

24 when they found the other strategies were not productive. When we moved to experiments in which each decision was made with a different set of parameters, we found that if two consecutive outcomes were fairly close together that subjects would try to implement the same outcome in those two consecutive decisions. For example, in experiments 008 and 009, the first equilibrium is 50 and the second is 45. In experiment 008 they chose 50 on the first decision and then tried at first to simply implement 50 on the second. Some individuals, however, put in lower responses until the group choice was brought down to 47. In experiment 009 the first decision was 45, instead of 50, raising some question about whether the second choice of 45 is not simply a hysteresis effect.

To control for the hysteresis effect we tried to make successive decisions have quite different equilibria: 21, 14, 39, 28 in experiments 004-007 and 013-014 and 50, 9, 45, 22 in experiments 010-012. In fact, we changed the order of the replications of Smith's (1979, 1980) parameters from 45, 9 to 9, 45 after observing the hysteresis effect in experiments 008 and 009.

Similar to the hysteresis effect is a strong tendency for individuals to compromise in order to foster group agreement, even when it is not necessary to do that; the mechanism will eventually bring them into agreement. Even after 2 or 3 decision, subjects will first profit maximize, probably in order to see the range of possible responses. Then, perhaps after trying out different strategies, they will go to an average of the first round responses, even when average responses are not profit maximizing for each individual. This can help the mechanism to converge to the equilibrium when the equilibrium is the average of the first round responses. But, when the equilibrium is not the average of the first round responses, it can lead the group to agree on some other outcome. This is evident in decisions 008d, 009d, 010d, and 012d. In that design the first average is 24, not 22. Three of the four groups converged to 24 and one converged to 23.

VI. Conclusions and Discussion of Future Research

The results presented in this paper demonstrate quite clearly that the B-H mechanism leads inexperienced subjects to the Lindahl equilibrium allocation of a public good (or an allocation very close to the Lindahl allocation), even under preference profiles already demonstrated to yield strong free riding under voluntary provision and to lead to a high proportion of failures under the Smith auction mechanism. The results are not as strong under one particular set of preference profiles, indicating more testing is necessary before any definitive conclusions can be drawn.

We are currently testing the B-H mechanism using experienced subjects with both CES and IMP-type parameters, and using our new lump-sum taxation scheme. We are currently concerned that the task in our first round of experiments may have been too simple: the inexperienced subjects were just presented with points along their budget lines and asked to respond with quantities of the public good. One group of experienced subjects will be given a separate sheet of paper at each decision, showing his or her entire payoff space in integers, a ruler to use to draw budget lines, and an additional set of written instructions on how the payoff information indicated on the PLATO screen corresponds to the payoff information on the page. This additional information will allow subjects to see their entire payoff spaces and may suggest ways to try to manipulate the mechanism to their advantages.

If the mechanism continues to perform well with experienced subjects, the next step is to replicate conditions more similar to those that would prevail in a naturally occurring situation. In the experiments thus far, the lump-sum tax and per-unit price are implicit in the payoffs presented to the subjects. They understand they are giving up x to get more Y, but that point is not presented as graphically as it would be in a naturally-occurring implementation. Using IMP preferences, we can generate induced demand curves for the public good and then present subjects with payment schedules for different quantities of the public good. The computer can still act to recalculate taxes, but it need not calculate a subject's profits. We also need to study the hysteresis effect and the tendency to compromise in experiments with experienced subjects.

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Table 1
Experimental Design

Experiment Number	Number of Decisions	Number of Participants	Form of Utility Functions	Location of Experiment
001	4	10	IMP (2 groups)	old lab
002	4	10	IMP (2 groups)	old lab
003	4	10	IMP (2 groups)	old lab
004	3	10	IMP (10 diff.)	old lab
005	4	10	IMP (10 diff.)	old lab
006	3	10	IMP (10 diff.)	old lab
007	3	10	IMP (10 diff.)	old lab
008	4	6	CES and CD (1)	new lab
009	4	6	CES and CD (1)	new lab
010	4	6	CES and CD (2)	new lab
011	4	6	CES and CD (2)	new lab
012	4	6	CES and CD (2)	new lab
013	2	10	IMP (10 diff.)	new lab
014	4	10	IMP (10 diff.)	new lab